A HISTORY OF

THE CAVENDISH LABORATORY

From a Photo by W. H. Hayles]

ENTRANCE OF CAVENDISH LABORATORY, SHOWING EXTENSION IN THE DISTANCE

A

HISTORY OF THE CAVENDISH LABORATORY

1871-1910

WITH 3 PORTRAITS IN COLLOTYPE
AND 8 OTHER ILLUSTRATIONS

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PREFACE

ON December 22, 1909, Sir J. J. Thomson completed the twenty-fifth year of his tenure of the Cavendish Professorship of Experimental Physics in the University of Cambridge. At the beginning of that year the suggestion was made by some of the Professor's immediate colleagues that the occasion should be celebrated in some way which would commemorate a tenure so long and so full of achievement.

There are many precedents, especially on the Continent, for such a celebration. In most cases it has taken the form of the publication of a volume of memoirs by various distinguished friends and admirers of the Professor, dealing with the subjects of their own special study. It seemed possible in the present case to publish a volume of a different character, and possibly of more lasting interest.

Sir J. J. Thomson is the third holder of the Cavendish Professorship: perhaps there is no post in the world which has been held successively by three men of such supreme and such varied genius. Their work is closely associated with the Cavendish Laboratory, and has made it known throughout the world.

It appeared, therefore, that Sir J. J. Thomson's twenty-five years' tenure of the Professorship might be fitly celebrated by the writing and publication of a History of the Cavendish Laboratory.

The various chapters have been written by those who have been closely associated with the Laboratory at different periods; and, at the special request of those responsible for the publication, Sir J. J. Thomson himself has contributed a chapter giving a general sketch of the last twenty-five years, a chapter which the Editors feel adds considerably to the interest of the volume.

The following extract from the letter addressed to contributors will indicate the intentions of the volume:—

'Much of the history of the Cavendish Laboratory is written in the transactions of learned societies. The republication of all this matter, even if it could be effected within the limits of a single volume, would serve no useful purpose. It is intended that the present volume should be the record, not of what work was done, but of how that work came to be done. It is thought that the evolution of the ideas which have inspired physical teaching and research in Cambridge, and the part played in that evolution by the many eminent men who have worked in the Laboratory, should be traced as far as possible; and it is hoped that the narration may be made in such a way as to be of interest even to those who are not professed students of our science. No doubt it will be found necessary and desirable to include some account of the more important researches, but the record of ideas and of personalities, rather than of experiment, should be the primary object.'

It will be seen that the relative importance attributed by the authors to the many aspects of the work of the Laboratory has varied considerably. But in so far as the view taken of the work of the Laboratory at various times is different, the differences doubtless represent real changes in the prevalent spirit. No attempt has been made by the Editors to secure homogeneity: their efforts have been directed solely to the avoidance of undue repetition.

For permission to reproduce the portraits of Maxwell

and of Lord Rayleigh we are indebted to the kindness of the owners of the originals, Sir Henry Roscoe and the President and Fellows of the Royal Society respectively. The photographs of the Laboratory and apparatus are the work of Mr. W. H. Hayles, the chief lecture assistant, for whose help our best thanks are due.

NOTE

THE appendices of memoirs and of those who have worked in the Laboratory are carried down to the end of 1909. A date enclosed in brackets in the text indicates that the memoir mentioned is included in the appendix under that date.

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A HISTORY OF THE CAVENDISH LABORATORY CAMBRIDGE

CHAPTER I

The Building of the Laboratory

The systematic teaching of practical physics is a modern development. Newton, Boyle, Young, and all the great experimenters of past generations, acquired their skill in the use of apparatus by solitary practice, or by helping some older master in his work. Until the second half of the nineteenth century was well begun, no teaching laboratory and no regular courses of instruction were known.

In England the pioneer work in systematised instruction was done in Oxford and London about 1867. At a still earlier time Lord Kelvin, then known as Professor William Thomson of Glasgow, had, it is true, taught many students to become experimenters, but his method was the earlier one of using their assistance in his own researches, an invaluable training, but a plan only possible for small numbers.

Professor R. B. Clifton was elected to the chair of Physics at Oxford in 1866. By borrowing a room from another department, he was able to start a small class in practical physics in 1867. The Clarendon Laboratory was

begun in 1868, first used for classes in 1870, and finished in 1872.

In October 1876 Professor G. Carey Foster also opened a single room as a teaching laboratory at University College, London, and in the following year Professor W. Grylls Adams began systematic practical instruction in another single room at King's College, where the apparatus from the museum of King George III., presented by Queen Victoria, was ready to form the nucleus of a teaching collection.

In the spring of 1868 Adams visited France and Germany, but found that the only laboratory where systematic instruction in practical physics was going on was that of Jamin at the Sorbonne. Here 'students were already engaged in the determination of Physical Constants'; elsewhere the only apparatus was that used by the professors in their own investigations.

It was thus with little help from precedents that Clifton, Foster, and Adams recognised officially that all serious students of Physics should pass through a practical course. Carey Foster writes:

> I was convinced that there could be no sound teaching of physics apart from practical work; students must have personal acquaintance with phenomena before they can profitably reason about them.

It would be difficult to frame a better justification for the modern physical laboratory.

The land on which the Cambridge Laboratories and Museums now stand was acquired by the University in 1762 in trust for the purposes of a Botanic Garden. In this Garden buildings to contain lecture-rooms were erected in 1786, and enlarged in 1833. These buildings, 'though inconvenient and ill-arranged, sufficed for more than thirty years for the use of the Professors of Anatomy, Physic,

Botany, Chemistry and Applied Mechanics, as the Jacksonian Professor came gradually to be designated.' [1]

In 1851 the Natural Sciences Tripos was established, and the growing number of students soon made further accommodation necessary. Moreover, in 1850 the Royal Commission on the University published the evidence of the Professors of Science, and public attention was drawn to the needs of the departments.

Meanwhile the Botanic Garden, now surrounded by houses, had become unsatisfactory. The removal of the collection to the present site, begun in 1846, was finished in 1852. There was some idea of 'disposing of the old Garden on building leases, or for the purpose of converting it into a market place, or for such other purposes as may appear advisable.' This scheme having fortunately failed, the University acquired from the Governors of the Botanic Garden full legal rights over the ground, and referred to the syndicate which had carried through the negotiations the difficult question of the construction of new museums and lecture-rooms.

The first scheme proved impracticable on account of its cost, and no further steps were taken until 1860, when the Council of the Senate reopened the question, wisely beginning with the study of finance. The result was that the foundation stone was laid by Professor Liveing on June 9, 1863, and in the course of the years 1864 and 1865 accommodation was provided successively for Zoology, Comparative Anatomy, Human Anatomy, Chemistry, Mineralogy, and Botany. The inception of the Physical Laboratory is thus described by Mr. J. W. Clark.[2]

The University next turned its attention to the want of instruction in Heat, Electricity and Magnetism, subjects to which

[1] *Architectural History of the University of Cambridge*, by Robert Willis and J. W. Clark, vol. iii. p. 157.

[2] *Loc. cit.* p. 181.

the scheme of examination for Honours in the Mathematical Tripos approved by the Senate, June 2, 1868, had given new prominence. A syndicate appointed November 25, 1868, to consider how these might best be taught, reported (February 27, 1869) in favour of founding a special Professorship, and of supplying the Professor with the means of making his teaching practical; in other words, of giving him a demonstrator, a lecture-room, a laboratory, and several class-rooms, with a sufficient stock of apparatus.

Here we have the conception of the future Cavendish Laboratory, but this scheme also was nearly wrecked on the rock of expense. The idea of the syndicate to raise funds by an increase in the capitation tax did not commend itself to the Senate, and the Long Vacation of 1870 found no decision made.

But at the beginning of the Michaelmas Term the Vice-Chancellor received the following letter from the seventh Duke of Devonshire, Chancellor of the University:

Holker Hall, Grange, Lancashire:
October 10, 1870.

MY DEAR MR. VICE-CHANCELLOR,—I have the honour to address you for the purpose of making an offer to the University, which, if you see no objection, I shall be much obliged to you to submit in such manner as you may think fit for the consideration of the Council and the University.

I find in the Report dated February 29, 1869, of the Physical Science Syndicate, recommending the establishment of a Professor and Demonstrator of Experimental Physics, that the buildings and apparatus required for this department of Science are estimated to cost £6300.

I am desirous to assist the University in carrying this recommendation into effect, and shall accordingly be prepared to provide the funds required for the building and apparatus, so soon as the University shall have in other respects completed its arrangements for teaching Experimental Physics, and shall have approved the plan of the building.

I remain (&c.)

DEVONSHIRE.

For some time it had been arranged that several new Professorships, of which that of Experimental Physics was one, should be founded as soon as funds were available. The heads of colleges now agreed that the sums required under the Cambridge Improvement Acts should be levied by assessment instead of being charged on the University Chest. This change lightened the burdens on the Chest, and on November 28 the Council of the Senate were able to propose the foundation of a Professorship of Experimental Physics. Their recommendation was approved on February 9, 1871. On March 8 James Clerk-Maxwell, M.A., of Trinity College, was elected the first Professor, and delivered his inaugural lecture on October 25 in the same year. He gave his first regular courses in the lecture-room of Professor Liveing, who, till the appointment of the Professor of Physics, had himself given instruction in Heat.

Meanwhile the Chancellor's letter had been published to the University, and a syndicate was appointed on March 2 to find a site and obtain plans and estimates. The syndicate consisted of the Vice-Chancellor, H. W. Cookson, Master of Peterhouse; W. H. Bateson, Master of St. John's College; Professors Adams, Humphry, Liveing, Maxwell, Miller; J. W. Clark, and Coutts Trotter. Before the syndicate considered plans for the building, Maxwell visited Oxford and Glasgow to study the arrangements made in those two universities for teaching practical physics.

On November 18 the syndicate reported in favour of the site adjoining Free School Lane, as being easy of access and far enough from the streets to be fairly free from vibration. They also submitted a design by Mr. W. M. Fawcett, and an estimate considerably in excess of the amount contemplated at first. They were able to state, however, that the Chancellor had approved the plans, and wished to extend

his munificent offer, and 'present the building complete to the University.'

The lowest tender, that of Mr. John Loveday of Kibworth, amounted to £8450. It was accepted by the Senate on March 12, 1872, and the building was nearly ready by the Michaelmas Term, 1873, during which term the lecture-room and laboratory for students were first used. The building was completed by Easter 1874, and was inaugurated formally on June 16, when the following letter of thanks was read in the Senate House by R. C. Jebb, the Public Orator, and with the Chancellor's reply is printed in full in the official *University Reporter*:

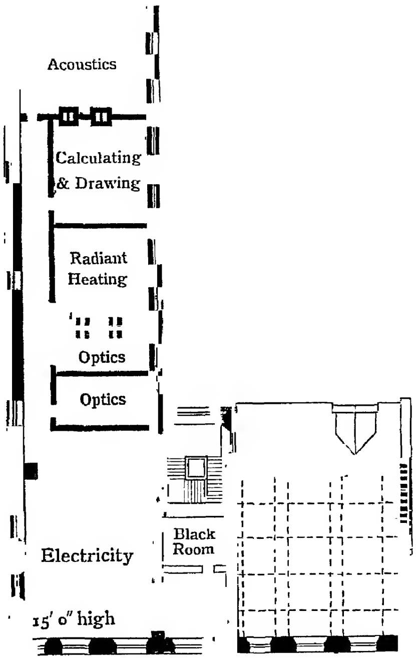
Illustrissimo Principi GULIELMO CAVENDISH, Duci Devoniæ, C.P.T.A., Academiæ Cantabrigiensis Cancellario, S.D.
Procancellarius Senatusque Cantabrigiensis.

Quom iam perfectum sit nobile illud Ædificium quod veram fore nobis scientiarum speramus officinam, tibi, Princeps illustrissime, simul grates agimus, simul gratulamur, quod ea te penes est laus, studia promovisse ad omnes Naturæ leges indagandas utilissima.

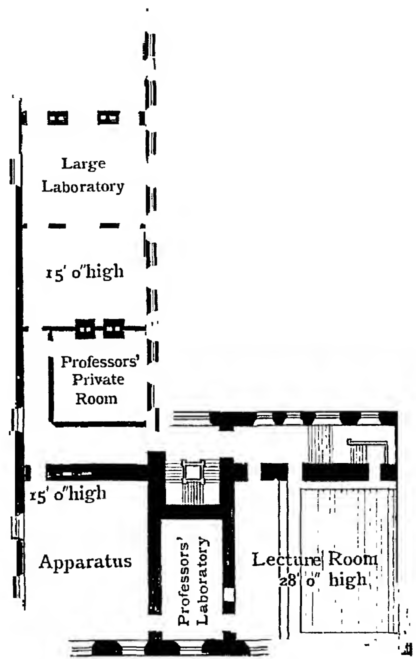
Sentimus Academiæ nostræ Cancellarium cui munus acceptum referimus vere regium, et suæ et suorum famæ felice quadam fortuna respondisse. Quem enim magis decebat novæ studiorum provinciæ tot talesque copias ministrare quam eum qui ab eiusdem disciplinæ campo tot tantasque victorias reportaverat? Unde vero convenientius poterat illis artibus succurri quam e tua domo, quæ in ipsis iampridem inclaruerat? Notum est Henricum Cavendish, quem secutus est Coulombius, primum ita docuisse, quæ sit vis electrica, ut eam numerorum modulis illustraret, adhibitis rationibus quas hodie veras esse constat.

Donum in nos contulisti te ipso dignum; videant—hoc nostrum est invicem optare—videant illi parietes opera tanto auctore non indigna, nomini clarissimo consentanea, amori nostro respondentia, iis denique votis idonea quæ, si quis scientiæ fautor est, te tuosque prosequentur.

Datum e Senaculo,
xvi. Kal. Quint. MDCCCLXXIV.



SECOND FLOOR PLAN



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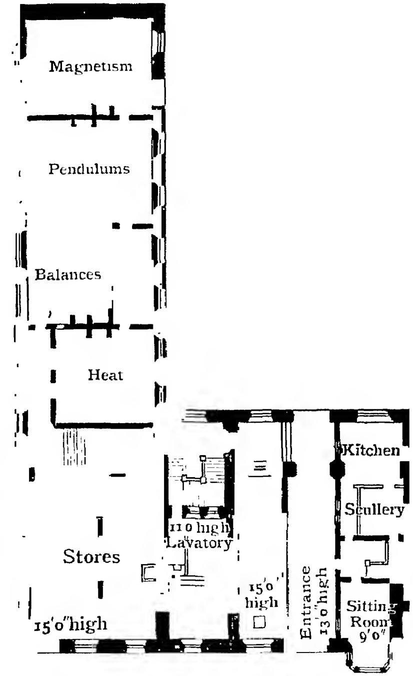

GROUND PLAN.

PLAN OF ORIGINAL LABORATORY

The Chancellor replied in the following terms:

Equidem viri ornatissimi, cum maxime Universitatis interesse arbitrer, ut quicquid opus sit ad scientias ex omni parte excolendas intra fines suos possideat, me felicem iudico quod ipsi officinam donare potuerim, quâ carere nequeunt, qui insigni si quæ alia in philosophia provinciæ operam daturi sint.

Quæ de Naturæ legibus cognita habemus, in hisce nostris temporibus magnum sane incrementum nacta sunt. Et hæc nostra Universitas scientias quæ istis innituntur legibus, ita demum efficaciter docere poterat, ita fines ipsarum, ut decebat, etiam ulterius promovere, si plures Lectores Publicos comparavisset, si thesauros coegisset, si ædificia et quæ vulgo musea appellant, idque sumptu non modico erexisset.

Hæc vero assecutis, fieri non potuit quin reditus vestri magnis oneribus premerentur; quibus quod ipse levandis, officinâ nempe Cavendisianâ extructâ, contulerim, id lucro mihi apponendum censeo. Neque alio modo philosophicam vestram, ut ita dicam, supellectilem libentius adaugere poteram, quam curando ut eæ scientiæ docerentur, quarum unus e maioribus meis, Henricus Cavendish—prout et vos me commonefactum voluistis—ipse magna ex parte fundamenta posuit. Quod porro in officinâ ipsâ nuncupandâ nomen eius commemorare dignati sitis, id grato animo accepi.

Tibi iam, Domine Pro-Cancellarie, officinæ clavem custodiendam committo, senatui ædificium do; cuius inter parietes operæ studebitur, haud mediocriter, nisi fallor, antiquam Universitatis nostræ famam provecturæ.

The Chancellor then proceeded to the new laboratory, unlocked the door and handed the key of the building to the Vice-Chancellor. An inspection of the building followed.

In his first report to the Museums and Lecture-Rooms Syndicate, dated May 1, 1874, Maxwell said that an experimental class was then at work on magnetic and optical measurements, and that the lecture-room had been in use since the previous October. He also noted the arrival of the standard electrodynamometer belonging to the Royal Society which, with the other instruments made for

the Committee of the British Association on Electrical Standards, was to be deposited in the laboratory.

At the opening ceremony the Chancellor had intimated his intention of presenting the principal instruments and apparatus required for the work of the laboratory which the University owed to his generosity. In his second report, Maxwell gives a list of the apparatus given by the Chancellor, and also of that deposited by the British Association. In his third report, he notes the gift by H. W. Elphinstone of the historic apparatus belonging to W. H. Wollaston, and, in his fourth report, he explains that the gift of the Chancellor was then complete, and adds optimistically that the collection comprised the whole of the 'instruments required by the present state of science'—a happy condition which has never since been attained. In this report, too, Maxwell states that—

> It has been felt that experimental investigations were carried on at a disadvantage in Cambridge because the apparatus had to be constructed in London. The experimenter had only occasional opportunities of seeing the instrument maker, and was perhaps not fully acquainted with the resources of the workshop, so that his instructions were imperfectly understood by the workman. On the other hand, the workman had no opportunity of seeing the apparatus at work, so that any improvements in construction which his practical skill might suggest were either lost or misdirected. During the present term a skilled workman has been employed in the laboratory, and has already greatly improved the efficiency of several pieces of apparatus.

Here we see the inception of the workshop, which has developed gradually into so large and important an addition to the laboratory.

It should be mentioned also that in 1887, on the death of Mrs. Maxwell, a considerable number of Maxwell's books were handed over to the Laboratory, and formed the nucleus of the present library. The collection, which has been

increased by the receipt of a few books formerly belonging to Sir George Stokes and by the preservation of some of the more important physical journals, was installed at first in a room on the top floor of the old building and transferred later to a room taken from the old Porter's Lodge.

The number of students doing experimental work in the laboratory steadily rose from a maximum of about twenty on any one day in 1877 to a total of sixty-two in 1882, and ninety, with ten doing research, in 1885. In 1888, Thomson, who had been appointed Professor in 1884, instituted practical classes for medical students, and this caused a sudden rise to 153 in the Lent Term, and in 1891 it became necessary to find further accommodation. Temporary provision was made by transferring the medical classes to the old dissecting room, but some permanent addition to the buildings was clearly needed.

In 1893 the houses along Free School Lane were assigned as a site for the extension of the Cavendish Laboratory. A syndicate, consisting of the Vice-Chancellor, the Provost of King's (A. Austen Leigh), the Master of Christ's (J. Peile), Professors Liveing and Thomson, and R. T. Glazebrook, reported, on November 1, 1894, that a suitable building could be erected for the sum of £10,000. In view, however, of the financial position of the University, they recommended that the ground floor only should be built at once, and that the two upper floors should be left for future construction. Towards the cost of this scheme Thomson offered the sum of £2000, accumulated from the fees of the students, and originally intended for the provision of more apparatus.

The large ground floor room, now used for medical students and for the more elementary course for Part I. of the Natural Sciences Tripos, was then built by Mr. Sindall,

of Cambridge, from the designs of Mr. W. M. Fawcett. The small lecture-room and the private room for the professor were added at the same time. The total cost was about £4000.

The extension was first used in the Lent Term of 1896, but, in the same report in which that fact was announced, another statement appeared, one that marked the inception of a movement which soon made even the new accommodation insufficient. In 1896 graduates of other universities began to come to Cambridge as advanced students, in accordance with a definite scheme, to undertake research under the inspiring guidance of J. J. Thomson.

While the undergraduate class work continued to extend, the growing number undertaking original experimental investigations soon made the pressure for room and apparatus acute. The need of further extension was accordingly noted in the Professor's reports for 1903 and 1905.

In 1906, Lord Rayleigh, who was Cavendish Professor from 1879 to 1884, and to whom the Nobel Prize for Physics had been awarded in 1904, offered to use £5000, the greater part of its proceeds, in building or helping to build a new wing of the Cavendish Laboratory, to satisfy the demands which spoke so well of Cambridge as a school of physics.

The new extension at once became practicable. The Museums and Lecture Rooms Syndicate recommended that the work should be taken in hand, and that the University should adopt Professor Thomson's suggestion and assign as a site for the building the frontage to Free School Lane, on the north side of the existing laboratory. This proposal was supported by the Special Board for Physics and Chemistry, and adopted by the Senate on November 6, 1906.

On November 22 a syndicate was appointed to obtain plans and estimates. It was composed of the Vice-Chancellor, the President of Queens' (T. C. Fitzpatrick); Professor Thomson, J. B. Lock, W. H. Macaulay, and W. C. D. Whetham, and was directed to report before the end of the Lent Term, 1907.

Accordingly the report appeared on February 21. Mr. W. M. Fawcett, who had designed the original building, had prepared a plan for the new extension, which provided a large basement and a number of small rooms on the second floor for research, together with a large lecture-room to seat 120 students, a library and chemical room, and a room for the demonstrators, on the first floor. By this means the room occupied by the library could be restored to the Porter's Lodge.

Mr. Fawcett estimated the cost to be between £7000 and £8300, the first sum on a basis of tenpence a cubic foot, and the second on a basis of one shilling.

To meet this outlay, Lord Rayleigh's benefaction of £5000 was available. Professor Thomson offered a second time to provide £2000 from the accumulation of the students' fees, and stated that he expected that no additional charge for maintenance would fall on the University.

This report was approved on March 14 of the same year, and the syndicate were authorised to obtain tenders. The lowest, that of Mr. Sindall, of Cambridge, proved to be £7135, including the sum of £500 allowed for heating.

From the point of view of the University, the financial position was thus so good that no opposition on that score was expected or experienced. The second report of the syndicate was accordingly approved on June 6, 1907, and the work was begun during the Long Vacation.

The building was completed during the ensuing Easter Term of 1908. Meanwhile, in succession to the eighth

Duke of Devonshire, Lord Rayleigh had been elected Chancellor, and his first act after his installation was to open the new wing of the laboratory built largely by his own munificence. On June 16, the thirty-fourth anniversary of the opening of the original laboratory, the Chancellor and a large company assembled in the new lecture-room.

On this occasion the proceedings were conducted in English, and the *University Reporter* dismissed the affair with the curt announcement that:

> At 4 P.M. the Chancellor went in state to the Cavendish Laboratory, where he declared the new extension of the laboratory to be open.

To supplement our memory of the ceremony we must therefore turn to the files of the *Times*, where we find a leading article on the work of the laboratory, and a report of the speeches delivered.

> The Chancellor said that his interest in the Cavendish Laboratory went back a good many years—to a date, in fact, before the laboratory existed, when there was little opportunity of acquiring the experimental art. He had some hand in urging Clerk-Maxwell to accept the first appointment of Cavendish Professor, and in Maxwell's life his letter, indiscreetly published, could be seen, using as an argument that there was no one in Cambridge at all fitted for the post. The five years he spent as Maxwell's successor were laborious years, lightened by the assistance of Dr. Garnett, and afterwards of Dr. Glazebrook and Dr. Shaw. Much of his experimental work related to the electrical units, which then were uncertain to as much as three or four per cent. In those days he began his lifelong friendship with Lord Kelvin, to whom he owed so much. Then, too, he met Helmholtz, the only physicist to be put in the same rank as Kelvin. Since his retirement from the Cavendish professorship, nearly a quarter of a century ago, the fame of the laboratory had spread far and wide. The large number of young men who had been trained in it would one and all be outspoken in their recognition of what they owed both in the way of direct teaching

and of stimulus to Professor Thomson. In these circumstances, and with the increased numbers of students, it was inevitable that more accommodation should be required, and that day they were inaugurating the new building, which he now declared open.

Professor Thomson called attention to the fact that it was their founder's day, the laboratory first having been opened on June 16, 1874. He thanked the Chancellor for his munificent benefaction, and all those who had been associated in the extension. He hoped that those using the laboratory would by their work show that they deserved the gift.

Upon the motion of the Vice-Chancellor, seconded by the Master of Trinity, a respectful vote of thanks was passed to the Chancellor, who, in reply, said there was nothing that appealed to him more directly than the work in which they had been engaged that day.

The new buildings came into full use at the beginning of the Michaelmas Term, 1908, and for the first time for many years the whole of the lectures in Physics in the University were concentrated in the Cavendish Laboratory.

CHAPTER II

THE CLERK-MAXWELL PERIOD

AT the time the Cavendish Laboratory was being built and equipped, the desire for a more efficient teaching of Physics had led to the independent introduction of systematic courses of practical work in many institutions; but as laboratory instruction had nowhere a recognised position in the University curriculum, each teacher was free to arrange his courses according to his inclinations. There was naturally a difference in the point of view, and hence in the system of instruction adopted, according as more or less importance was being attached either to the advance of knowledge or to the training of students, though I imagine that everywhere both objects were kept in mind. It is of historical interest to compare in this respect the ideals of different teachers, and fortunately Clerk-Maxwell explained, in an Introductory Lecture delivered in October 1871, the position which he intended the Cavendish Laboratory to occupy in the scientific activity of Cambridge. I cannot do better than quote the more significant passages of this address. After pointing out the necessity for cultivating and diffusing sound dynamical ideas all the more, when '*such indeed is the respect paid to science, that the most absurd opinions may become current, provided they are expressed in language, the sound of which recalls some well-known scientific phrase,*' he marks the distinction between experiments of illustration and experiments of research in these words:

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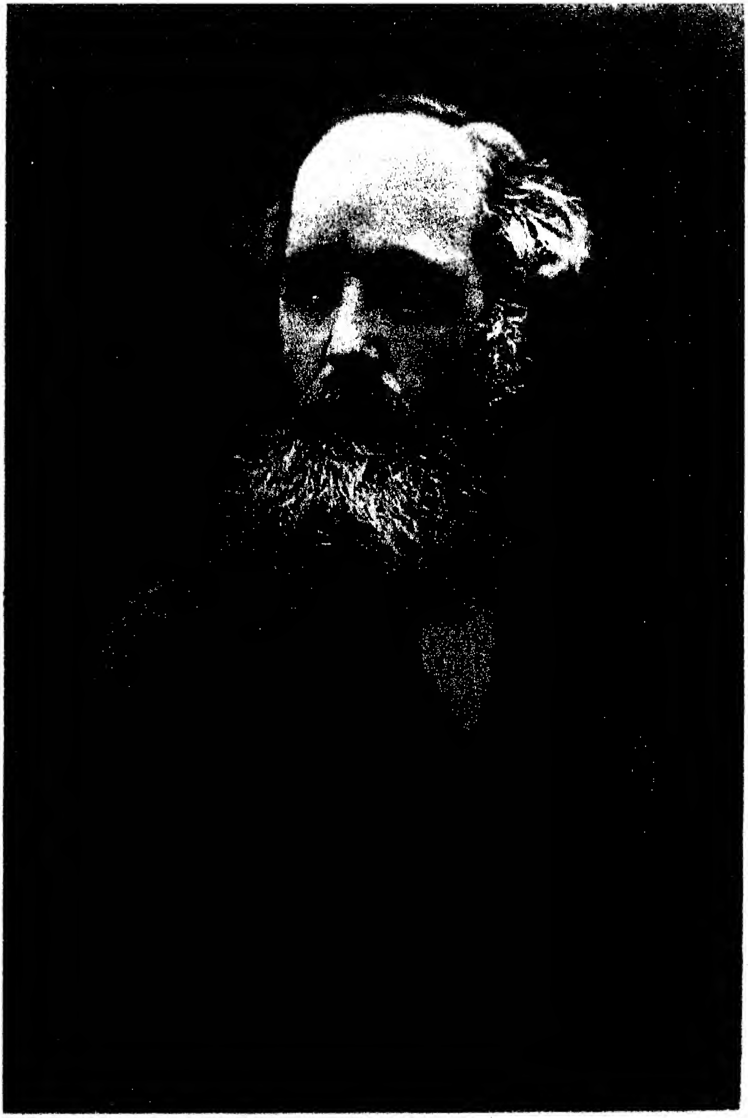

JAMES CLERK-MAXWELL.

CAVENDISH PROFESSOR 1871-1879.

From a photograph in the possession of Sir Henry Roscoe, reproduced by kind permission of the owner.

Experiments of illustration may be of very different kinds. Some may be adaptations of the commonest operations of ordinary life, others may be carefully arranged exhibitions of some phenomenon which occurs only under peculiar conditions. They all, however, agree in this, that their aim is to present some phenomenon to the senses of the student in such a way that he may associate with it the appropriate scientific idea. When he has grasped this idea, the experiment which illustrates it has served its purpose.

In an experiment of research, on the other hand, this is not the principal aim. It is true that an experiment, in which the principal aim is to see what happens under certain conditions, may be regarded as an experiment of research by those who are not yet familiar with the result; but in experimental researches, strictly so called, the ultimate object is to measure something which we have already seen—to obtain a numerical estimate of some magnitude.

Experiments of this class—those in which measurement of some kind is involved—are the proper work of a Physical Laboratory. In every experiment we have first to make our senses familiar with the phenomenon; but we must not stop here, we must find which of its features are capable of measurement, and what measurements are required in order to make a complete specification of the phenomenon. We must then make these measurements, and deduce from them the result which we require to find.

This characteristic of modern experiments—that they consist principally of measurements—is so prominent, that the opinion seems to have got abroad that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry on these measurements to another place of decimals.

If this is really the state of things to which we are approaching, our laboratory may perhaps become celebrated as a place of conscientious labour and consummate skill, but it will be out of place in the University, and ought rather to be classed with the other great workshops of our country, where equal ability is directed to more useful ends.

But we have no right to think thus of the unsearchable riches of creation, or of the untried fertility of those fresh minds

into which these riches will continue to be poured. It may possibly be true that, in some of those fields of discovery which lie open to such rough observations as can be made without artificial methods, the great explorers of former times have appropriated most of what is valuable, and that the gleanings which remain are sought after rather for their abstruseness than for their intrinsic worth. But the history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers. I might bring forward instances gathered from every branch of science, showing how the labour of careful measurement has been rewarded by the discovery of new fields of research, and by the development of new scientific ideas. But the history of the science of terrestrial magnetism affords us a sufficient example of what may be done by Experiments in Concert, such as we hope some day to perform in our laboratory.

The whole address is full of interest, but I must refrain from quoting more than the following passage, which refers more directly to Maxwell's ideal of the functions of a University Laboratory:

But what will be the effect on the University if men, pursuing that course of reading which has produced so many distinguished Wranglers, turn aside to work experiments? Will not their attendance at the laboratory count not merely as time withdrawn from their more legitimate studies, but as the introduction of a disturbing element, tainting their mathematical conceptions with material imagery, and sapping their faith in the formulæ of the textbooks? Besides this, we have already heard complaints of the undue extension of our studies, and of the strain put upon our questionists by the weight of learning which they try to carry with them into the Senate House.[1] If we now ask them to get up their subjects not only by books and writing, but at the same time by observation and manipulation, will they not break down altogether? The Physical Laboratory, we are told, may perhaps

[1] The locality in which the examinations were held.

be useful to those who are going out in Natural Science,[1] and who do not take in Mathematics, but to attempt to combine both kinds of study during the time of residence at the University is more than one mind can bear.

I quite admit that our mental energy is limited in quantity, and I know that many zealous students try to do more than is good for them. But the question about the introduction of experimental study is not entirely one of quantity. It is to a great extent a question of distribution of energy. Some distributions of energy, we know, are more useful than others, because they are more available for those purposes which we desire to accomplish.

We shall therefore arrange our lectures according to the classification of the principal natural phenomena, such as heat, electricity, magnetism, and so on.

In the laboratory, on the other hand, the place of the different instruments will be determined by a classification according to methods, such as weighing and measuring, observations of time, optical and electrical methods of observation, and so on.

The determination of the experiments to be performed at a particular time must often depend upon the means we have at command, and in the case of the more elaborate experiments this may imply a long time of preparation, during which the instruments, the methods, and the observers themselves are being gradually fitted for their work. When we have thus brought together the requisites, both material and intellectual, for a particular experiment, it may sometimes be desirable that before the instruments are dismounted and the observers dispersed, we should make some other experiment, requiring the same method, but dealing perhaps with an entirely different class of physical phenomena.

Our principal work, however, in the laboratory must be to acquaint ourselves with all kinds of scientific methods, to compare them, and to estimate their value. It will, I think, be a result worthy of our University, and more likely to be accomplished here than in any private laboratory, if, by the free and full discussion of the relative value of different scientific procedures, we succeed in forming a school of scientific criticism, and in assisting the development of the doctrine of method.

That Maxwell did not underrate the difficulties that

[1] Taking their degrees in the Natural Sciences Tripos.

stood in his way is shown in the following passage, taken from a letter dated March 15, 1871, addressed to Lord Rayleigh and kindly placed by him at my disposal:

Many thanks for your good wishes with respect to the new professorship. I always looked forward to it with much interest, tempered with some anxiety, when it was merely to be erected in the University. I now take your good wishes as personal to myself, and my anxiety has developed into responsibility. I hope you will be in Cambridge occasionally, for it will need a good deal of effort to make Exp. Physics bite into our University system, which is so continuous and complete without it.

To wrench the mind from symbols and even from experiments on paper to concrete apparatus is very trying at first, though it is quite possible to get fascinated with a course of observation as soon as we have forgotten all about the scientific part of it.

If we succeed too well, and corrupt the minds of youth till they observe vibrations and deflexions and become Senior Ops. instead of Wranglers, we may bring the whole University and all the parents about our ears.

It was clear that the Cavendish Laboratory could not expect to attract many students at the beginning, as it could only hope to enlist those who, having passed the Mathematical Tripos, had also sufficient aptitude for, and interest in, practical work to enjoy that combination of mental and sensory activity which comes naturally to a few, but requires in most cases a certain effort and training. The ground was to some extent prepared when physical subjects, such as the theory of electricity and magnetism, heat and capillary action, were reintroduced into the Mathematical Tripos after an interval of twenty-four years. At the first examination under the new regulations, which came into force in 1873, Maxwell acted as the additional examiner, who was to take special charge of the physical subjects. Among the candidates in that year was William Garnett, who passed as Fourth Wrangler, and so much impressed Maxwell by the knowledge of physics displayed in his

papers, that he offered him the demonstratorship in the laboratory. This position Garnett held until Maxwell's death.

The distinction of being the first student who worked in the Cavendish Laboratory belongs to W. M. Hicks, of St. John's College, (now Professor of Physics at the University of Sheffield,) who left Cambridge on his appointment as Principal of Firth College, Sheffield, a position he occupied until the University was founded in 1905, when he was appointed its first Vice-Chancellor. Prof. Hicks was good enough to give me an account of his work at the laboratory, from which I may quote the following passage:

> Beyond resistance measurements and using the Kew magnetometer to get H with all corrections and under the best conditions, I do not remember doing any of the 'regular' things. When Lippmann published the principle of his electrometer, I designed one on that basis, and made one, and the getting that to act properly was worth any amount of routine measurements. As illustrating the effect of Maxwell's teaching, I was fired with the desire of measuring experimentally the velocity of propagation of electromagnetic waves (1874), and designed a piece of apparatus to prove its existence, depending on the action of two coils, a large one and a very small one, on the same circuit, acting on a light magnetic needle at a point where their combined force was zero. As the large coil was much further away, I supposed that when the current was suddenly started—or suddenly stopped—the effect of one at the needle would disappear before that of the other, and the needle would give a kick. Of course nothing came of it, but the practice was worth a great deal to me, and it is interesting as showing the kind of atmosphere in which one was working round Maxwell.

J. E. H. Gordon (Caius College, Junior Optime, 1875), who next joined the laboratory, undertook, under Maxwell's guidance, research work with the object of accurately determining some electrical constants. He was, I believe, the first student who was able to submit for publication

the result of work carried out in the Cavendish Laboratory. A paper dated April 30, 1875, and read before the Royal Society on June 17 of that year, contained a measurement for water of the rotation of the plane of polarisation of light in a magnetic field of unit strength, and was subsequently withdrawn and replaced by a more complete investigation, in which the more strongly active bisulphide of carbon was used as medium. The wave-length of the light used, which had been left undefined in the first paper, was now fixed within narrow limits by separating from a continuous spectrum the region coincident with the light sent out by a thallium flame. The final paper was published in the *Transactions of the Royal Society*, 1877.

In a letter to *Nature*, dated Cavendish Laboratory, June 29, 1875, Gordon described experiments on the then newly discovered effect of light on the electric conductivity of selenium. He points out that the effect depends on the molecular condition, one sample examined by him being insensitive to light. A further research carried out by him at the Cavendish Laboratory appeared in the *Philosophical Transactions* for the year 1879, and dealt with the specific inductive capacities of different substances—more particularly those of transparent dielectrics. By means of these measurements, it was hoped to test the electromagnetic theory of light, which in the form given to it by Maxwell requires that the square of the refractive index should be equal to the dielectric capacity multiplied by the magnetic permeability. The complication resulting from the dispersive properties of the medium could not at that time be satisfactorily dealt with, and hence the agreement of theory with observation was not quite satisfactory.

The above description gives only a very incomplete account of Gordon's great scientific energy. He had established a laboratory at his home in Pixholme, near

Dorking, and it was there that the researches which he began at Cambridge under Maxwell's inspiration were generally completed. He was one of the pioneers of electric lighting enterprise, and published a treatise on electric lighting. For some time he acted as Assistant Secretary to the British Association, and died in 1893.

George Chrystal (Second Wrangler, 1875), now Professor of Mathematics of the University of Edinburgh, was one of the earliest workers in the Cavendish Laboratory. The research in which the accuracy of Ohm's law was for the first time submitted to a severe test arose out of a paper which I presented to the British Association at Belfast in the year 1874; and some account of the initial stages of the work which ultimately led to my residence in Cambridge may therefore take its place here. It was, so far as I remember, mainly in consequence of conversations with Balfour Stewart on the nature of electric currents, that I came to examine the experimental evidence of the truth of Ohm's law, and found that it rested only on a very slender basis. It has been said, with partial truth, that the accuracy of Ohm's law is a matter of definition, the ratio of the electromotive force to the current defining the resistance; but it may for convenience be held to include something more than what is contained in this definition. We may take it that the law implies that the ratio defining the resistance does not depend on the intensity of the current or on the electromotive force, provided all other conditions remain unchanged. The main difficulty in testing the law lies in the impossibility of keeping the temperature constant, when the current intensity is altered to any great extent. Previous to Chrystal's experiments there was no evidence that the law was more than a rough approximation, nor is there any theoretical reason why it should hold accurately. On the contrary, theoretical investigations have generally

indicated a breakdown of the law for strong currents; thus our present ideas of the nature of electric conduction might lead us to expect that the number of electrons taking part in the process of conduction depends, to some extent, on the strength of the electric field.

It was with the idea of finding some change in the molecular conditions of conductors, probably of a directive quality, brought about by the current itself, and affecting their conductivity, that I undertook some experiments in the physical laboratory of the University of Göttingen. The galvanometer at my disposal for the purpose had a heavy magnet in the form of a thick circular disc, the front of which was polished and acted as a mirror. Alternating currents could be produced by a rotating magnet which was attached to the disc of a siren, kept in motion by a blast of air: the note given out by the siren served as a measure of the angular velocity. The experiment consisted in measuring the deflexions of the galvanometer magnet by a very weak current, on which a much stronger alternating current could be superposed. It was found that the addition of the latter always increased the deflexions. This result was brought to the notice of the meeting of the British Association at Belfast in 1874, and Professors Maxwell, Everett, and myself were appointed a Committee to investigate experimentally the accuracy of Ohm's law. In this manner originated the important research which was carried out in the Cavendish Laboratory by George Chrystal, and led to a confirmation of Ohm's law with an accuracy thus commented upon by Maxwell, who had designed the experimental arrangement:

> It is seldom, if ever, that so searching a test has been applied to a law which was originally established by experiment, and which must still be considered a purely empirical law, as it has not hitherto been deduced from the fundamental principles

of dynamics. But the mode in which it has borne this test not only warrants our entire reliance on its accuracy within the limit of ordinary experimental work, but encourages us to believe that the simplicity of an experimental law may be an argument for its exactness, even when we are not able to show that the law is a consequence of elementary dynamical principles.

The cause of the anomalous result which had led to the appointment of the Committee was further investigated by Chrystal, and ascribed by him to the change in the longitudinal magnetisation of the galvanometric needle, caused by the electro-magnetic effect of the alternating currents in the galvanometer coil, when the needle is displaced from the central position. Chrystal's explanation appears convincing and could certainly be applied to the effects observed by him, which to all appearances were identical with mine, but there is still a remnant of doubt in my mind whether it covers the whole case. According to Chrystal, the permanent current only acts in so far as it displaces the galvanometer needle, and if this be true, the increase of the displacement should appear equally when the needle is displaced by an outside magnet. This test was actually applied by me at the time, but an increased deflexion was only observed when the original displacement was caused by the current through the galvanometer. Unless there is here an error of experimentation, there is still something left to be investigated.

The same volume of the 'British Association Reports' which contains the testing of Ohm's law also contains a communication by G. Chrystal and S. A. Saunder on the results of a comparison of the British Association Units of Electrical Resistance. A number of coils had been prepared by the British Association Committee of Electrical Standards to serve as standards to which the ohm could be referred. These coils—originally deposited at Kew Observatory—

were transferred to the Cavendish Laboratory, and in view of the possible change in resistance of any or all of them, it was important to compare them with each other at frequent intervals. The comparison was difficult because accurate temperature determinations were almost impossible owing to the wires being embedded in solid paraffin. In the work initiated by Chrystal, and subsequently so ably continued by Fleming and Glazebrook, we have the first stage of the series of investigations which in Lord Rayleigh's time covered the whole range of electrical measurements and made the Cavendish Laboratory the chief centre for establishing and maintaining accurate standards of electrical units, a position it held until the foundation of the National Physical Laboratory allowed this part of its activity to be transferred to its present home.

The experiments on Ohm's law gave rise to a correspondence between Clerk-Maxwell and myself, and, further inspired by a study of his two volumes on 'Electricity and Magnetism' which had recently appeared, I felt a wish to come more directly under his influence. On my writing to him during the spring of 1876, asking for permission to work in the Cavendish Laboratory, I received an answer which is so characteristic of Maxwell's kindness of heart and encouraging disposition, that I hope its publication may be considered excusable, though its interest is mainly personal. It runs as follows:

May 3, 1876.

DEAR SIR,

It would do us all great good if you were to come and work in the Cavendish Laboratory. The very prospect of your coming has caused all our pulses to beat about one per minute quicker.

The Schuster effect and the anti-Schuster effect have long been the objects of our regard, but we look forward to the time when these terms will have lost their significance as applied to

the phenomena of electric conduction, and when every department of physics will have a recognised Schusterismus.

I know of no rule which would interfere with your working here (provided you do not let the gas escape), and members of the electrical committee of the B. A. have, by the desire of the Founder (the present Duke of Devonshire), liberty to make electrical experiments.

I shall probably be in Scotland from early in June till October, but I shall be here all May, and Mr. Garnett is generally here a good deal in summer.

Mr. Garnett has made a very beautiful resistance coil of German silver wire $\frac{1}{500}$ inch diameter wound between the threads of a screw cut in ebonite and covered with a glass tube. The resistance is about 7000 ohms. This is to test the Schuster effect with an induction coil. When we wish to get the residual effect of the alternate currents we shunt the galvanometer through a large resistance wound double so as to have very little self-induction. The alternate currents are thus almost entirely confined to the shunt while the residual effect is divided between the galvanometer and the shunt in the proportion of their conductivities.

But Chrystal's result is as follows: If the resistance of a conductor (of German silver, platinum or iron) for infinitely small currents is 1 ohm, and if its section is 1 square centimetre, then (provided its temperature is kept the same) its resistance is not diminished by so much as the $\frac{1}{10^{12}}$ part when a current of a farad[1] per second passes through it, or, in other words, when the electromotive force between its extremities is 1 volt.

I am at present occupied with electric conduction through hot air, metallic vapours, flames, &c.

Yours very truly,

J. CLERK-MAXWELL.

To discuss my migration to Cambridge, Clerk-Maxwell asked me to stay at his house for a few days, during which I became acquainted with Chrystal and Garnett, and met

[1] The names of electrical units, at the time this was written, were in a state of flux. In the second edition of the *Electricity and Magnetism* (vol. ii. p. 247) Maxwell calls the unit of quantity: 'a farad (charged to a volt).'

Lord Rayleigh for the first time at dinner in Trinity College Hall. I began work in October 1876, intending at first to undertake a systematic investigation of diamagnetism, the details of the experimental arrangements of which had been determined after a correspondence with Maxwell. This subject had suggested itself to me in the hope of throwing light on Weber's theory of diamagnetism, there being a vague idea in my mind of a possible connexion between Weber's molecular currents and the spectra of gases. Ultimately the interest of mainly spectroscopic questions gained the upper hand. It seemed to me to be more immediately important to study the spectra of metalloids on account of the exaggerated importance attached by some astronomers to the apparent absence of evidence indicating the existence of metalloids in the sun. For this purpose it was necessary in the first instance to investigate the conditions under which one and the same element could give several independent spectra. The discovery of multiple spectra belongs to Plucker and gave rise to a good deal of discussion. While on the one hand some spectroscopists went so far as to deny it altogether, others accumulated the number of spectra of a body almost indefinitely by disregarding the ordinary precautions of purifying their materials. It seemed best to select one element and to investigate thoroughly: I chose oxygen because this body had yielded very discordant results in previous investigations. In connexion with this research I am tempted to mention a curious example of an observation depending on some unknown factor which cannot afterwards be recalled. In the course of the investigation it was found that the characteristic spectrum of the negative glow of oxygen persisted in the neighbourhood of the electrode for a few seconds after reversal of the current, when, therefore, in consequence of the reversal, the cathode had been

converted into an anode. The conclusion was drawn that the spectrum of the negative glow was due to a molecular combination which could exist independently for a short time. The experiment was kept going during some time; Maxwell saw it and was interested in it, and several of my co-workers in the laboratory as well as visitors witnessed the effect, which appeared in several of the tubes which I used. Some years later M. G. Salet of Paris wrote to me to say that he had tried the experiment without success. As soon as an opportunity offered I set up the apparatus again, but entirely failed to reproduce the effect; I have since made one or two unsuccessful attempts, and am at a loss to account for this apparent inconsistency of nature, unless the result was due to the tubes in the original experiment having been in constant use for several months and possibly having their electrodes in some peculiar condition.

There was another spectroscopic investigation carried out in the laboratory at the same time. Arthur W. Clayden and Charles T. Heycock had noticed, while working at a course of lectures on Spectrum Analysis given by the Professor of Chemistry, that the spectrum of indium, when obtained by passing the spark from an induction coil between electrodes of the metal, gave a number of additional lines not noticed in the flame spectrum or when the spark is taken from the chloride. The additional lines were measured and the interesting fact recorded that one of the lines shown by the chloride and also by the nitrate was entirely absent when the spark was taken from the metal.

An early attendant at Maxwell's lectures, Sedley Taylor of Trinity College, then writing a book on 'Sound,' constructed an instrument named the Kaleidophone, in which a soap film was made to act as elastic membrane. Set into vibration by the appropriate sound, variations

of thickness of the membrane in different parts of it became apparent by beautiful colour effects.

A research was undertaken by A. W. Sunderland (Mathematical Tripos 1876) on anomalous dispersion in fuchsin. Its object—so far as I remember—was to determine the refractive indices as close to the absorption band as possible. The work remained unfinished, and Sunderland died a few years later.

The name of J. A. Fleming, now Pender Professor of Electrical Engineering, has already been mentioned. It was the desire to work under Maxwell which induced him to give up the science mastership at Cheltenham College and settle down at Cambridge. Beginning work in 1878, and taking up Chrystal's work of comparing the British Association units of resistance, he designed for this purpose a special bridge, and determined the most probable value of the ohm derived from the original measurements of the British Association Committee. This work is frequently alluded to in the reports of that body.

Another important research conducted in the laboratory was that by R. T. Glazebrook on the form of the wave surface in biaxial crystals. It is needless here to set forth the difficulties which beset the undulatory theory at the time, and though these difficulties had then already been removed by Maxwell's electromagnetic theory, it was still considered necessary to protect the orthodox view against attack. Independently of all theory, however, it was important to determine experimentally with the highest attainable accuracy whether Fresnel's equations correctly represent the form of the wave surface. Glazebrook—having submitted Fresnel's surface to a severe experimental test—arrived at the result that it is nearly accurate, slight deviations from it being possibly due to an effect of dispersion. It is perhaps worthy of remark

that the volume of the *Philosophical Transactions* which contains Glazebrook's paper contains also two other researches communicated by Maxwell. The three together, covering 140 pages, make up nearly one-third of the volume—a proportion which is increased if we include a paper by Maxwell himself, and shows that the Cavendish Laboratory had already, early in its career, established its position as a centre of research.

One of the features of the investigations carried out in the Cavendish Laboratory during Maxwell's time was the wide range of the subjects over which they extended. In this respect the laboratory has perhaps never since reached the same degree of catholicity, an advantage—for so it must be considered—due probably to students coming to the laboratory for a definite purpose of working out either an idea of their own or one which had been suggested to them, but not in all cases by the same person. An important investigation was undertaken, for instance, at the instigation of the Meteorological Council. At a meeting of that body on July 7, 1879, the following resolution was passed:

> That a comparative series of experiments be made to test the indication of Dines' hygrometer by the chemical method of determining the absolute amount of water present in the air, and that, in connexion with this subject, comparisons be also made with the wet and dry bulb thermometers, with Regnault's hygrometer, and de Saussure's hair hygrometer.

The Chairman, Professor H. J. S. Smith, and Professor Stokes were instructed to arrange for having such a series of experiments carried out. Stokes, with a view to having the work done at the Cavendish Laboratory, put himself into communication with Maxwell, who wrote to W. N. Shaw, then working in the Physical Laboratory of Berlin. As the date given indicates, Maxwell was already stricken by

his fatal illness when Shaw began the work, but it deserves to be recorded as a proof of Maxwell's insight into the capabilities of the men with whom he was—though, perhaps, only slightly—brought into contact, that he selected Shaw as being the most suitable man to carry out this research, and thus first introduced the present Director of the Meteorological Office to the branch of physics in which he soon rose to eminence.

Among the work still planned in Maxwell's time, but carried out only after his death, Poynting's research on the constant of gravitational attraction deserves a prominent place. Poynting had been acting as demonstrator to Balfour Stewart, who was then making experiments at Owens College, Manchester, to find, if possible, a change in weight in lead discs when first placed vertically and then horizontally on the balance plate. In trying to calculate the attraction of surrounding bodies which were sufficiently near to produce a disturbing effect when the position of the lead disc was changed, it occurred to Poynting that a large mass which could be introduced or removed from underneath the balance pan might affect the apparent weight sufficiently to allow the gravitational constant to be determined. After preliminary experiments which showed the feasibility of the method, Poynting sent an account of the work, so far as he had carried it out, to the authorities of Trinity College, Cambridge, as a fellowship dissertation, and Maxwell acted as referee. Having obtained the Fellowship in 1878 Poynting resigned his position in Manchester in the succeeding year in order to complete his experiments in the Cavendish Laboratory, where his stay was soon cut short by his appointment to the Professorship of Physics at Mason College, Birmingham. Though Maxwell died soon after Poynting came into residence, he had taken a great interest in the early stages

of the experiment, and I remember Joule, at a meeting of the Literary and Philosophical Society of Manchester, quoting a letter, in which, in alluding to the proposal to determine the gravitational constant by weighing, Maxwell had remarked: 'You see that the age of heroic experiments is not yet past.'

What has been said will already have shown that Maxwell took an active part in every research that was carried out in the laboratory. He used to come round daily, enter the various rooms, and say a few words, discussing results or making suggestions, but he was most at ease when he spoke about the subjects which occupied his mind at the time; about these, he would speak very freely. On several occasions he spoke to me of Boltzmann's theorem regarding what we now call the equipartition of energy, and though Maxwell accepted the conclusions arrived at, it was only with some hesitation, because he was a little afraid of what the theorem might ultimately lead to. I remember especially being impressed by his statement that, if true, the theorem ought to be applicable to liquids and solids as well as to gases. Maxwell often showed a certain absent-mindedness: a question put to him might remain unnoticed, or be answered by a remark which had no obvious connexion with it. But it happened more than once that on the following day he would at once refer to the question in a manner which showed that he had spent some time and thought on it. I never could quite make up my mind whether on these occasions the question had remained unconsciously dormant in his mind until something brought it back to him, or whether he had consciously put it aside for future consideration, but it was quite usual for him to begin a conversation with the remark: 'You asked me a question the other day, and I have been thinking about it.' Such an opening generally led to an interesting and original treatment of the subject.

Those who knew Maxwell will remember many of his quaint sayings. The two following more especially remain in my mind: the first is characteristic, the second carries a sting which he not often allowed himself to show. A question was asked of Maxwell whether a certain so-called physical law was true or not. His answer was: 'It is true in the three days but not in the five,' alluding to the then existing division of the Mathematical Tripos into three days of elementary papers and five days in which a more advanced knowledge was required of the candidate. The second remark arose out of some criticism I had made on a paper, which I said I did not consider had been worth publishing. This elicited from Maxwell the general aphorism: 'The question whether a piece of work is worth publishing or not depends on the ratio of the ingenuity displayed in the work to the total ingenuity of the author.'

During the last few years of his life Maxwell took an intense interest in repeating many of the experiments of Henry Cavendish, whose writings he was preparing for publication, and he was specially fascinated by the manner in which Cavendish anticipated subsequent discoveries, by converting himself into a galvanometer. There was at the time no known effect of an electric current which could be utilised in an instrument of precision, and Cavendish had the original idea of measuring a current by estimating the intensity of the physiological shock which he felt when the current was suddenly sent through his body, the experiment being arranged so that two currents were declared to be equal when the shocks they produced appeared to be equally intense. Maxwell set up the necessary apparatus, and everyone who came to the laboratory at the time had to submit himself to the trial of being forcibly convinced that the method was sufficiently accurate to give consistent results. Occasionally a visitor failed to appreciate the

beauty and efficiency of the method, and I remember more especially a young American astronomer expressing in severe terms his disappointment that, after travelling on purpose to Cambridge to make Maxwell's acquaintance and to get some hints on astronomical subjects, the latter would only talk about Cavendish, and almost compelled him to take his coat off, plunge his hands into basins of water and submit himself to the sensation of a series of electrical shocks.

In connexion with the publication of Cavendish's researches, Maxwell designed an improved form of the well-known experiment of the sphere and hemispheres, proving that the whole charge of a conductor resides at its surface. The two hemispheres were permanently kept together so that they formed an outer shell to the sphere, which was placed inside, resting on a short piece of wide ebonite tubing. There was an opening to the shell covered by a disc hinged to the shell. This disc, which acted as a lid, could be lifted by means of a silk wire, and when closed connected the sphere and the shell by means of a short wire attached to it. In the experiment the shell and enclosed sphere were charged while in metallic connexion; the lid was opened, the shell discharged, and the potential of the sphere measured by a quadrant electrometer. It is known that the absence of the charge of the sphere under these circumstances proves the law of the inverse square, and Maxwell concluded from the experiment that if the law of force follows the law r^{-2+q}, q must be between $\pm 1/21600$. These experiments were carried out by Dr. (now Sir) Donald MacAlister (Senior Wrangler 1877), now Principal of Glasgow University.

Maxwell's lectures were carefully prepared, but badly attended. Professor Fleming writes:

Maxwell generally gave a couple of courses of University

lectures per year, which, however, were shockingly neglected, and I remember my surprise at finding a teacher whom I regarded as in the very forefront of knowledge lecturing to three or four students as his only audience. In fact during one term Professor Maxwell gave a course of splendid lectures on Electro-Dynamics, the only audience being myself and another gentleman whose name I think was Middleton, at that time a fellow commoner of St. John's.

The lectures referred to in the last instance must have been delivered in one of the last two terms before Maxwell's death, as Middleton only came into residence in 1879.

Mention should be made of the Rede lecture delivered by Maxwell on the 'Telephone' at the Senate House in 1878. The preparations for the lecture were all made at the laboratory, where wires were laid from the basement to the attics, with telephones attached to the ends. The Professor at one end and the demonstrator at the other tried to communicate with each other. The main difficulty, according to Maxwell's account, consisted in preventing the demonstrator's voice reaching him through the brick walls in a sufficiently subdued form to give the electric current a chance to compete.

It has already been mentioned that William Garnett acted as demonstrator during the whole period of Maxwell's directorship of the Laboratory. The demonstrator superintended the ordinary Laboratory practice, though Maxwell himself frequently instructed students, especially those who were intending to undertake research work after having gained a preliminary experience in physical measurements. On the question of Laboratory teaching Garnett writes:

Maxwell's idea at first was to attach to the Laboratory a small band of graduates, each of whom would undertake a definite piece of work after going through a short course of training in measurement. For its educational value he held in high repute the Kew Magnetometer, as it afforded practice not only in reading scales

and making adjustments, but also in time observations, counting the beats of a watch while observing the vibrating magnet.[1] At first all students coming to the Laboratory were invited to measure the horizontal component of the earth's horizontal magnetic force, making their own calculations before proceeding to any other piece of work. It was not then contemplated that the Laboratory would be much used by undergraduates.

The day of practical examination in Physics for the Natural Sciences Tripos was not yet. It was not till about three years after the opening of the Laboratory that elementary lectures suitable for medical students and other undergraduates were started. These lectures soon became popular and formed some of the largest Science classes in the University. When Lord Rayleigh became Professor he thought they occupied too much of the attention of the staff and interfered with the practical work of the Laboratory; so he abolished them, and for some time there were no practical Physics lectures available for medical students.

I introduced into the Laboratory workshop a few engineers' and joiners' tools, and later on two instrument makers were frequently employed. When Professor Willis died, and Professor James Stuart took his chair, the workshops of the Engineering Department were started, the two instrument makers were taken over by Professor Stuart, and the more important instrument work went with them. The little workshop in the Cavendish Laboratory was the starting-point of the University Mechanical Engineering Department.

My own work was mainly to lecture to the more elementary students, and render mechanical assistance to the Laboratory students and to those engaged in their own researches. I was occupied a good deal in devising experiments for lecture illustration and making suitable apparatus. Whenever I came upon a new piece of work I showed it to the students, but did not publish it; and while I assisted in a good many of the investigations which Maxwell suggested, I never embarked on a continuous research of my own.

For several years Maxwell would not open the Laboratory to women. At last he gave permission to admit women during the Long Vacation, when he was in Scotland, and I had a class who were determined to go through a complete course of electrical

[1] The Kew magnetometer is still one of the instruments which form part of the training of all candidates for the second part of the Natural Sciences Tripos.

measurements during the few weeks for which the Laboratory was open to them.

The Laboratory examination was introduced in a tentative way into the Natural Sciences Tripos in 1874, when in a paper which also contained questions on Chemistry and Mineralogy, students were given an opportunity to show that they could find the focal length of a lens, the time of vibration of a magnet, or the electric resistance of a wire. Maxwell acted as examiner in that year. Three similar simple questions were set in 1875, when Professor W. G. Adams examined. In the following year the Natural Sciences Tripos was divided into two parts, taken in June and December respectively. In the second part a separate paper was set, containing eight Laboratory questions, of which not more than four were to be attempted.

The instruction in practical work was not systematised to the extent it is now, and students were left a good deal to themselves to cope with their difficulties unaided. In this manner independence was gained and experience acquired more widely than is possible with the system of over-instruction which is current in many laboratories at the present time, but the advantage was not always appreciated by the student who was nervously apprehensive of the dangers of the coming examinations.

The following extract from a letter received from Mr. A. P. Trotter, now electrical adviser to the Board of Trade, shows one point of view and may serve to give an idea of the different types of instruments which students had opportunities to become familiar with during their course of instruction. I think on the whole that the instruments suffered more than the students by their being so freely allowed to come unchaperoned into mutual contact.

Either I or someone else made a light-hearted attempt at measuring the current of the Holtz machine by means of a high-resistance Elliott galvanometer. Somebody scolded the culprit, but I could not see the enormity of the offence. Among the apparatus that I can remember using was a small spectroscope with which I measured the angles and refractive index of a prism. I had a book of trigonometrical tables in my hands for the first time. I knew all that the extras of the Little-go required as to trigonometry, but I could not understand why the sines and cosines stopped at 45°. I wanted an angle of about 60° and thought it must be in a second volume! I remember your allowing me to see the double sodium line in your big spectroscope, as a treat. I spent several days with the Kew magnetometer, and Garnett explained how to calculate the moment of inertia of the magnet—a quantity of which I had never heard.

I remember trying to measure a low resistance by the logarithmic decrement of a low-resistance Elliott galvanometer. The galvos were ordinary Thomson type. There was an optical bench for experimenting with lenses, and a good heliostat. The big cathetometer with a 24–25 vernier bothered me a good deal. There was a gramme dynamo with an ingenious driving arrangement, the construction of which I did not understand until two or three years after I went down.

The resources of the Cavendish Laboratory were in the first instance intended to benefit Cambridge students and Cambridge graduates. When I arrived with the intention of working in the Laboratory under the superintendence of the professor, but unwilling to submit myself to the routine of a degree course, there was no pigeon-hole in the University organisation into which I could be fitted conveniently. I paid no fees, I was no member of the University, and officially, I imagine, did not exist. I am all the more grateful for the kindness and hospitality privately shown me by those members of the University—rapidly increasing in numbers—who, looking beyond its mere degree-giving functions, were striving to establish schools of research in various branches of science; some of them had reached eminence already, others were only

standing at the beginning of their career. By them the new-comer, though unknown, was welcomed as a sign that their efforts were beginning to be successful. At the end of the first term—mainly owing to the action of William Garnett—I was admitted to St. John's College as a fellow commoner. Some difficulty, I believe, was felt about the gown I should be entitled to wear, as the fellow commoner's garb with its gold tasseled cap was thought to be inappropriate; ultimately, at the suggestion of the Master of St. John's, I was instructed to wear a bachelor's gown without strings. Thus I became the first research student at Cambridge.

In sketching the early history of the Cavendish Laboratory I have given a fragmentary but, I hope, sufficient account of the work which was carried out, but I am afraid my description will fail to convey sufficiently to the reader how powerfully Maxwell's personality dominated the Laboratory and united those who worked in it. It is not easy to define Maxwell's position as a teacher. His influence on scientific thought was mainly exerted through his writings, and reached its full effect only after his death; while the power which he exercised by personal contact was principally a moral one. Having originated new and fertile ideas in all branches of Physics, Maxwell might easily have found students eager to work out in detail some problem arising out of his theoretical investigations. This would have been the recognised method of a teacher anxious to found a 'school'; but it was not Maxwell's method. He considered it best both for the advance of science, and for the training of the student's mind, that everyone should follow his own path. His sympathy with all scientific inquiries, whether they touched points of fundamental importance or minor details, seemed inexhaustible; he was always encouraging, even when he thought a

student was on a wrong track. 'I never try to dissuade a man from trying an experiment,' he once told me; 'if he does not find what he wants, he may find out something else.' It was the seriousness with which he discussed all ideas put before him by his students that, from the beginning, gave the Laboratory its atmosphere of pure and unselfish research.

CHAPTER III

LORD RAYLEIGH'S PROFESSORSHIP

CLERK MAXWELL died in 1879 and was succeeded by Lord Rayleigh. The Professorship of Experimental Physics had been founded for Maxwell, and, according to the regulations under which it was held, was to 'terminate with the tenure of office of the Professor first elected unless the University by Grace of the Senate shall decide that the Professorship shall be continued.' By a Grace of November 20, 1879, the professorship was continued, and Lord Rayleigh, in response to a very general wish, expressed his willingness to undertake the duties of the post if he were appointed. Accordingly he was elected on December 12, 1879, and took up the work in the Lent Term of 1880, lecturing on the use of Physical Apparatus. The regulations of the professorship required him to reside for eighteen weeks during the academical year and to deliver one course of lectures in each of two terms, forty lectures at least during the year.

The number of his class in this first Lent Term was sixteen, and among the sixteen were the following whose names appear in the first six Wranglers in the Mathematical Tripos of 1881: Forsyth,[1] Heath,[2] Steinthal, Dodds,[3] Pollock.[4]

During the year W. N. Shaw and R. T. Glazebrook

[1] Sometime Sadlerian Professor of Mathematics.
[2] Principal of the University of Birmingham.
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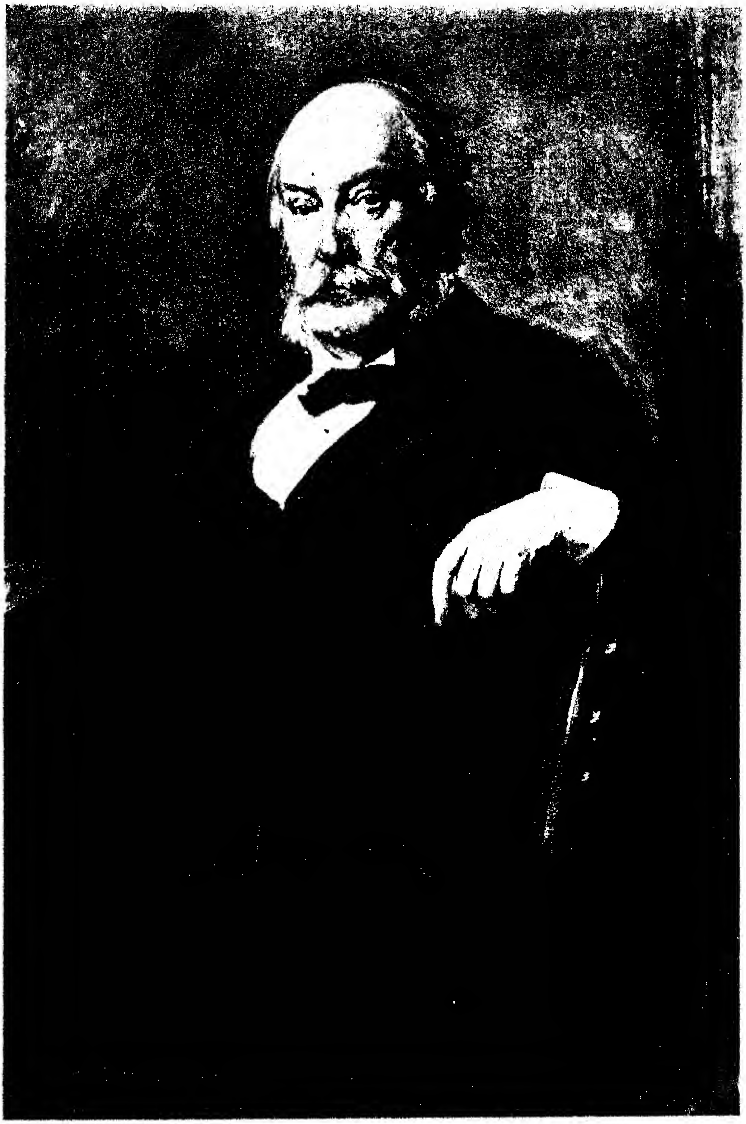

LORD RAYLEIGH.

AFTER THE PAINTING OF SIR GEORGE REID, R.S.A.

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became Demonstrators of Physics, and the courses of regular instruction in Practical Physics, which have been a marked feature of the Cavendish Laboratory since that time, were begun.

The notice for the Michaelmas Term 1880 is to the effect that—

> The Professor of Experimental Physics will lecture on Galvanic Electricity and Electro-Magnetism. Dr. Schuster will give a weekly course on Radiation. Mr. Glazebrook will give an elementary course of demonstrations on Electricity and Magnetism, Mr. Shaw will give a course of demonstrations on the Principles of Measurement and the Physical Properties of Bodies.
>
> The Laboratory will be open daily from 10 to 4 for more advanced practical work under the supervision of the Professor and Demonstrators for those who have had the necessary training. Courses of demonstrations will be given during the Lent Term on Heat and Advanced Electricity and Magnetism, during the Easter Term on Light, Elasticity, and Sound.

These various courses of demonstrations were each for two hours, three days a week. The number of students attending the lectures and demonstrations at first was small. In the Lent Term, 1880, sixteen attended Lord Rayleigh's lectures, eighteen the elementary demonstrations on Heat, and fourteen the advanced demonstrations on Electricity and Magnetism. The numbers, however, soon increased and in the Lent Term 1884 the elementary courses were duplicated.

At the same time J. H. Randell of Pembroke and J. C. McConnell of Clare were appointed Assistant Demonstrators.

In the Easter Term of 1884, Lord Rayleigh's last term as Professor, the numbers in attendance at the lectures and classes were as follows:

The Professor's lectures	14
Mr. Glazebrook's demonstration	47
Mr. Shaw's demonstration	23

In the Lecture list for the Lent Term 1880 the names of three persons, the Professor of Experimental Physics, the Demonstrator of Experimental Physics, and Mr. Trotter, occur as giving lectures in Physics, and the total number of courses for the term was five, while on the list for the Lent Term of 1884, the last term in which Lord Rayleigh lectured, the number of courses was ten.

In the Natural Sciences Tripos list of December 1879, twenty-five names occur, while in June 1884 there were twenty names in Part II. of that Tripos and sixty in the list of Part I.

In 1882 women were admitted for the first time to the Tripos Examinations, and the question of the position women students were to hold in the Laboratory was settled by the decision that all classes and demonstrations were to be open to students of Girton and Newnham Colleges on the same terms as to members of the University.

The few statistics given indicate the progress of the classes during Lord Rayleigh's tenure of the Professorship; a list of names of the students will show that no inconsiderable number have since distinguished themselves by their work as teachers and investigators; the time he held the post was too short to permit of the growth of any large body of research students. His own work and the organisation of the Laboratory for teaching and research are the most marked results of these five years.

The Laboratory had been built and equipped in a most generous manner by the Chancellor, but continuous additions to equipment were a necessity, and the funds at the disposal of the University did not permit of much expenditure in this direction. One of Lord Rayleigh's first acts was to remedy this by asking for donations from a number of his friends for this purpose. The sum raised amounted to about £1500. Of this sum the Duke himself gave £500 and Lord

Rayleigh £500; Lord Powis contributed £100. Resident members of the University were not asked to help, and indeed the news that more was needed to supplement the Duke's generous gift was somewhat of a surprise to many.

Maxwell, in his Report to the University in 1877, had written to the effect that the Chancellor had completed his gift to the University by furnishing the Cavendish Laboratory with apparatus suited to the present state of Science, and this had been varied to the statement that the Chancellor had completely provided the Laboratory with apparatus suited to the present state of Science—a very different matter. During the last few years of his life Maxwell had himself provided what was wanted, expending many hundreds of pounds in this manner, and the demands made on University funds had been but small. He had also in the spring of 1874 presented to the University all the apparatus in his own possession, while in accordance with a resolution passed at the Edinburgh Meeting of the British Association in 1871, all the apparatus provided for the work of the Electrical Standards Committee was deposited at the Laboratory, the apparatus remaining the property of the Association.

Still, the number of students at work up to 1879 was not large, and when demonstrations and practical work began to be organised on a considerable scale it became clear that there were many gaps to be filled. Lord Rayleigh's fund was a necessity.

The teaching of Practical Physics in those days was an undeveloped art. Kelvin had his laboratory at Glasgow, Carey Foster at University College, Guthrie at South Kensington, and Clifton at Oxford, but the time given to practical work was not long, and there was comparatively little systematic endeavour to teach fundamental physical

principles to the ordinary student by means of experiments which he performed himself.

Perry in 1880, in a lecture before the Society of Arts, called attention to Ayrton's work in Japan, and at the same time Ayrton himself was just beginning his labours at the Finsbury Technical College.

Nor were there books to help the student. Kohlrausch's 'Physical Measurements' and Pickering's 'Elements of Physical Manipulation,' published in 1873, of which a second edition appeared in 1878, were almost all that were available. Professor Pickering, in his preface to the first edition, expresses clearly the need and the method of meeting it. After stating that 'This rapid spread of the laboratory system of teaching Physics, both in this country and abroad, seems to render imperative the demand for a special text-book to be used by the students,' and explaining the method in which the book was to be used, he continues:

Perhaps the greatest advantage to be derived from a course of physical manipulation is the means it affords of teaching a student to think for himself. This should be encouraged by allowing him to carry out any idea which may occur to him and as far as possible devise and construct with his own hands the apparatus needed.

The method of conducting a Physical Laboratory for which this book is especially designed and which has been in daily use with entire success at the Institute[1] is as follows. Each experiment is assigned to a table on which the necessary apparatus is kept and where it is always used. A board called an indicator is hung on the wall of the room, and carries two sets of cards opposite to each other, the one bearing the names of the experiments, the other those of the students. When the class enters the laboratory each member goes to the indicator, sees what experiment is assigned to him, then to the proper table where he finds the instruments required, and with the aid of the book performs the experiment.

[1] The Massachusetts Institute of Technology.

Instructions as to calculating and entering up the results follow, and Professor Pickering continues:

By following this plan an instructor can readily superintend classes of about twenty at a time and is free to pass from one to another answering questions and seeing that no mistakes are made.

The plan is now well known. Most of those now engaged in physical teaching and research here and in America have been brought up in accordance with it. Much of the work of Lord Rayleigh and of those who helped him during the tenure of his Professorship was devoted to its introduction at the Cavendish Laboratory, and I am glad to avail myself of the opportunity of indicating the source from which it came.

Such a plan has its drawbacks, and in some cases the assistance given to the student is far too great. The apparatus on the table is so nicely adjusted, the connexions to be made so clearly indicated, that all that remains to be done is to press the button and the answer will come out. Still, with a large class of students to deal with, all of whom have to be taught certain leading laws and facts in a limited time, organisation of the kind is absolutely necessary; the more interesting alternative method by which I was taught Practical Physics some three years previously is impossible.

After a very brief introduction to the measurement of electromotive force and resistance I was told by Professor Maxwell to observe the behaviour of a set of large gravity Daniell cells which were in use for Chrystal and Saunder's experiments on Ohm's law, and as the instrument assigned to me for the measurement of the electromotive force was the well-known large pattern of Thomson quadrant electrometer, the difficulties, at any rate for a beginner, were not small.

Our work as Demonstrators was most interesting; one of the first tasks was to find a Laboratory attendant. Pullin, the man who had helped Maxwell, died soon after Lord Rayleigh's appointment, and George Gordon filled his vacancy. He was a shipwright from Liverpool, of whom I had heard through his attendance at some Science and Art Department Classes conducted by a friend, and he filled his post admirably until Lord Rayleigh's retirement, when he was taken to Terling.

Much of the apparatus used in the Laboratory was his handiwork, and, though he was far from being a skilled instrument-maker, he soon acquired from the Professor some of his own capacity for attending closely to essentials, and the results were admirable.

We had but little experience at first to guide us, and beyond the two books I have mentioned there were none. Shaw had worked under Warburg at Freiburg, and Helmholtz in Berlin. My own knowledge of a Physical Laboratory was limited to what I had learnt under Maxwell at the Cavendish. We drew on Kohlrausch's book freely; its value to students of Physics has long been recognised, and without it our difficulties would have been almost insurmountable; but neither it nor Pickering were quite suitable for a class-book, and so a series of MS. note-books was prepared each dealing with one experiment, describing the apparatus actually in use in the Laboratory, the method of making the experiments, and in particular of entering up the results. The books were corrected from time to time and at length found their way into print, ultimately being published in the 'Text-books of Science Series' as a book on Practical Physics by Glazebrook and Shaw. This covered the ground of the elementary portion of the work, that required for the first part of the Natural Sciences Tripos. In compiling it we were met by the want of a standard

book on Theoretical Physics to which we could readily refer, and in consequence the book contains more theory than would now be necessary; much that then seemed to need elaboration and explanation has now become a matter of common knowledge.

The advanced classes were worked in a somewhat similar way, but from the beginning an attempt was made to introduce students to original work and important researches by endeavouring to repeat such of these as were possible with the means at our disposal. Students were expected to read the memoirs referred to and in their work to reach a high order of accuracy; the books in which they wrote out their results became in time a valuable collection, and the pleasure of teaching the stamp of men who attended was very great.

The Laboratory books show that in the Lent Term of 1881 my class on Advanced Electricity and Magnetism contained fourteen names, and among these I find the following: J. M. Dodds, Peterhouse; L. H. Edmonds and S. L. Hart, St. John's; W. B. Allcock and Edward Hopkinson, Emmanuel; A. R. Forsyth, R. S. Heath, Theodore Beck, and G. W. Johnson, Trinity; J. Ryan, King's; and R. Threlfall, Caius. And in the work we had the Professor's help in the best possible of ways. The course was planned out and the syllabus was arranged with him after most careful consideration and discussion; we could go to him in any difficulty and were sure of cordial sympathy and advice, and of his ready assistance whenever that proved necessary; otherwise we were free to teach as seemed best and to develop the work in those directions which appeared to us to be most necessary.

Each of the two Demonstrators was on duty three days a week, and during those three days there was not much time for anything besides the work of the class. On

the other three days we were free for research work, College duties, or private teaching, as the case might be. The conditions of work were delightful. The pleasure of developing a comparatively new line of teaching, the sympathy and support of the Professor, the interest in seeing the work grow and the realisation of the importance of the fruits that were to come, all combined to make those years 1880–1884 a period of real happiness to the staff of the Laboratory.

During the same period the method of conducting the practical examination in Physics which is now so familiar was developed.

The increase in the number of candidates rendered a definite scheme necessary. Lord Rayleigh examined in 1882 and 1883, Mr. Shaw in the two following years, while at the same time the annual College and Scholarship Examinations grew in size and importance, and careful organisation was required.

At first it was desirable to attach more value than is at present necessary to the knowledge how to attack a question, and candidates were required to write out some fairly complete account of their methods before they began their practical work. The ordinary methods of experiment are now so much better known than they were, that this need is not marked.

Again a system of choice had to be devised which, while it did not unduly limit the candidate, yet rendered it possible to examine a large number of men simultaneously with such apparatus as was available. The question of the proper distribution of marks between the paper work and the practical examination raised other difficulties; indeed, it was not quite easy to devise any satisfactory system of marking.

The Long Vacation classes filled another somewhat

special place in the scheme of teaching. During the 'Long' the Laboratory was generally closed to the ordinary first part Tripos student; it was the time that the demonstrators and advanced students had for their own research work; but in order to encourage mathematicians to enter for Science a special course of demonstrations was usually given, open to those who had taken a degree in the Mathematical Tripos and to some few others. With a small number of selected men it was possible to get through a great deal of preliminary work in an interesting manner, and put the students in a position to take up the more advanced classes in the ensuing Michaelmas Term.

But the most marked results of the Rayleigh period at the Cavendish are his own splendid researches. In order to render these comprehensible to those who have no intimate knowledge of physics, a long digression will be necessary.

The realisation of the importance of exact measurement as an end to the advancement of knowledge is a marked feature of the progress of physical science in the latter part of the last century.

Dynamics had advanced in great measure owing to the effort to solve the problems of astronomy, and in this science definite ideas as to the measurement of the quantities considered were common. It was realised that three independent quantities were involved in the specification of the various quantities dealt with by the science, and the three which it was simplest to adopt were length, time, and mass. Where kinematics, the geometry of motion, alone is concerned, as in the measurement of velocity or acceleration, a knowledge of the first two quantities, length and time, is sufficient: if considerations of force or energy enter into the problem, the third quantity,

mass, is involved. Each physical quantity is measured in terms of a unit of its own kind, but in any consistent dynamical system these various units can all be expressed in terms of the three fundamental units. Thus a point has unit velocity when it describes unit distance in unit time, while the unit of force is a force which, applied to a unit of mass, increases its velocity in the unit of time by the unit of velocity.

Such a system of dynamical units is called an absolute system. By the use of an absolute system the statement of the relations between the various quantities is greatly simplified and awkward numerical factors are avoided in the equations.

Again, it is found convenient to speak of the 'dimensions' of these various derived units in terms of the fundamental units.

Thus, if the unit of length is increased, since a body moving with the unit of velocity traverses the unit of length in the unit of time, the unit of velocity is increased in the same proportion. Velocity is thus said to be of one dimension in length. On the other hand, if the unit of time is increased, the velocity will be proportionately decreased: the body has longer to describe the same distance and moves more slowly. Thus velocity is of dimension −1 in time.

The unit of acceleration involves the unit of time twice, for acceleration is measured by the velocity added per unit of time: thus it is of dimension 1 in velocity and −1 in time. But velocity has dimension 1 in space and −1 in time: hence acceleration has dimensions 1 in space and −2 in time.

Force, again, involves the unit of mass: the unit of force applied to the unit of mass gives it unit acceleration: it is measured absolutely by the product of mass and

acceleration: it is, therefore, of dimensions 1 in mass, 1 in space and -2 in time.

So far we have not defined our fundamental units: they may be any convenient length, time, and mass—provided we adhere to these throughout—and we shall get a consistent absolute system. It has, however, generally been agreed for physical purposes to take the centimetre as the unit of length, the gramme as the unit of mass, and the second as the unit of time. The absolute system derived from these is known as the C.G.S. system of units.

Again, any such system of absolute units can be extended from dynamics to other branches of physical science. Gauss and Weber in Germany pointed this out and suggested systems of electrical and magnetic units so derived.

When the two plates of a battery are joined by a wire an electric current flows through the wire, producing, among other things, magnetic force in its neighbourhood, and heat in the wire itself; the current can be measured by the magnetic force to which it gives rise at a point near the wire. The name 'electromotive force' was given to the agency to which the current is due, and Ohm showed that in a given conductor under fixed physical conditions the current is proportional to the electromotive force. This constant ratio of electromotive force to current is called the resistance of the conductor.

But so far there is no relation between current, resistance, and electromotive force on the one hand and the fundamental units of dynamics on the other; to obtain this relation certain physical laws must be considered.

Various considerations lead to the conception of a 'quantity of electricity,' and it is clearly consistent to say that unit current is traversing a wire when the unit quantity

of electricity crosses each section in one second. But how are we to define unit quantity of electricity?

Where two bodies are electrified it is found that a force of attraction or repulsion, as the case may be, is called into play between them: according to Coulomb's experiments, which have been accurately verified since his day, this force, in the case of two electrified particles, is found to be proportional to the product of their charges and inversely proportional to the square of the distance between them. If the charges are alike in sign, the force is one of repulsion; if they are unlike, it is one of attraction. This law may thus be written

$$F = qq'/r^2,$$

where F is the force, q, q' the charges, and r the distance between them.

We are led hence to a definition of the unit charge or unit quantity of electricity. For, suppose that the two charges are equal, so that $q = q'$: let them be placed at unit distance apart, and suppose that it is found that the force between them is the unit of force, so that $r = 1$ and $F = 1$. Then $qq' = 1$ or, since $q = q'$,

$$q^2 = 1 \quad \text{and} \quad q = \pm 1.$$

Hence the charge on each particle must be the unit quantity. Thus the unit quantity of electricity is that quantity which, when placed at unit distance from an equal quantity, repels it with the unit of force. This is known as the 'electrostatic unity of electricity,' being based on the statical action between two charged bodies.

A similar law holds for magnetic poles; thus there is a repulsion between the two magnetic poles of strength m, m', placed at a distance r apart, which is equal to mm'/r^2. The unit magnetic pole is a pole of such strength that it will repel an equal pole placed at unit distance with unit force.

If the force and distance are measured in C.G.S. units, so too will the unit quantity of electricity or the unit magnetic pole be given in C.G.S. units.

In one respect the above statements require modification. If the electrical force between two bodies be measured when the bodies are in air and the bodies be then immersed in some other insulating material, it will be found that the attraction or repulsion, whichever it was, has been altered in a ratio which is always constant for the same two media. Experiment shows us that the ratio

$$\frac{\text{Force in given insulating medium}}{\text{Force in air}} = \frac{1}{K},$$

where K is a constant for the medium. Therefore

$$\text{Force in medium} = \frac{\text{Force in air}}{K} = \frac{qq'}{Kr^2}.$$

K is known as the electrical 'specific inductive capacity' or dielectric constant of the medium, and is a constant for the medium which in nearly all cases is greater than unity.

In the same way, when dealing with a magnetic pole, the more complete law is—

$$F = \frac{mm'}{\mu r^2},$$

where μ is the magnetic inductive capacity or 'permeability. For most media μ is found to be the same as for air. If we limit ourselves now to air, for which both K and μ are unity, and assume that these quantities are merely numbers and have no dimensions, we can, from the above results, find the dimensions of a quantity of electricity. For we have

$$q^2 = Fr^2,$$

taking a case where $q = q'$. So that the square of a quantity of electricity has the same dimensions as a force

multiplied by the square of a distance. The dimensions of a force are $[M][L][T]^{-2}$, using the capital letters $[M]$, $[L]$, $[T]$ to denote the units of mass, length, and time. Hence we have

$$[q]^2 = [M][L]^3[T]^{-2} \quad \text{or} \quad [q] = [M]^{1/2}[L]^{3/2}[T]^{-1}.$$

Thus the dimensions of a quantity of electricity are $\frac{1}{2}$ in mass, $\frac{3}{2}$ in length, and -1 in time. A magnetic pole has the same dimensions.

But we have said already that an electric current exerts force on a magnetic pole, and this action may be made the basis of another system of electrical units. The action is called electromagnetic, and the system of units derived from it 'the electromagnetic system': the units of this system will be related definitely to those of the electrostatic system.

It follows from experiments that, if a piece of thin wire of length l is bent to form the arc of a circle of radius r and a current of strength i is made to flow through the wire, the force on a magnetic pole of strength m placed at the centre of the circle is given by the equation

$$F = mil/r^2.$$

From this we might get the result that if the length of the wire is unity, the radius of the arc unity, and the strength of the pole unity, so that $m = 1$, $r = 1$, $l = 1$, then $F = i$. Hence, if F is also unity, we have $i = 1$.

Thus the unit of current is one which, when flowing in the arc of a circle of unit length and unit radius, exerts unit force on a unit pole at the centre of the circle, and the unit quantity of electricity is the quantity which is carried across each cross section of the wire in unit time.

But this unit quantity, the electromagnetic unit, is not necessarily the same as the electrostatic unit, and indeed

it is many times greater. The exact numerical relation between the units can, of course, be determined by experiment only, but by considering the dimensions of the two units we can arrive at an important conclusion as to the nature of the result which experiment will give. For we have

$$i = \mathrm{F}r^2/ml;$$

putting in the dimensions of the various quantities we find

$$[i] = [\mathrm{M}]^{1/2}[\mathrm{L}]^{1/2}[\mathrm{T}]^{-1};$$

and since $\mathrm{Q} = it$, where t is the time the current is flowing,

$$[\mathrm{Q}] = [\mathrm{M}]^{1/2}[\mathrm{L}]^{1/2},$$

where [Q] stands for the electromagnetic unity of quantity. If we take the ratio of these two units we have

$$\frac{[q]}{[\mathrm{Q}]} = [\mathrm{M}]^{1/2}[\mathrm{L}]^{3/2}[\mathrm{T}]^{-1} \Big/ [\mathrm{M}]^{1/2}[\mathrm{L}]^{1/2} = \left[\frac{\mathrm{L}}{\mathrm{T}}\right].$$

Thus we see that, if we change our units of length and time, we change the number representing the ratio of the electrostatic to the electromagnetic unit, in the same ratio as the unit of velocity. But the number of electrostatic units in one electromagnetic unit is the reciprocal of the number representing the ratio of the electrostatic to the electromagnetic unit; and the number which measures the velocity of any system changes as the reciprocal of the unit of velocity. Hence, if we change our units of length and time, the number of electrostatic units in one electromagnetic unit changes in the same ratio as the numbers which measure velocities. So that, if that number is equal to the number representing the velocity of some system with one set of units of length and time, it will be equal to the number representing the same velocity with any other set of such units. That is to say, we can represent the

number of electrostatic units in one electromagnetic unit as the velocity of some system, and it becomes a matter of some importance to find out what is this velocity, which is usually denoted by 'v'. Experiments on this point have led to the interesting result that 'v' is the velocity of light in a vacuum—a numerical coincidence which led Maxwell to his electromagnetic theory of light.

And now the question arises, How are these units to be realised in practice? A definite current is flowing in a wire, or a condenser contains a definite charge: what steps must be taken to tell how many of these units, electrostatic or electromagnetic as the case may be, the wire is carrying or the condenser is charged with?

Before attempting to deal with this, it will be useful to consider how certain other fundamental quantities, electromotive force and resistance, are to be defined on the absolute system; and for the present it will be convenient to consider quantities to be measured in electromagnetic units, though what follows can easily be made to apply to electrostatic units also.

We have spoken of electromotive force as the agency causing the flow of a current in a conductor: it is important to define it more precisely. When a current flows in a wire work is done, and this work is measured, at least in cases when no change is produced in the electromagnetic field about the wire, by the heat generated in the wire. Thus it is found by experiment that the ratio of the work done to the quantity of electricity conveyed round the circuit is a constant, and this constant is called the electromotive force in the circuit. Thus, if W be the work done per unit of time, t the time that the current i has been flowing in the circuit and Q the quantity of electricity carried, and E the electromotive force, the total work done is Wt, and experi-

ment shows that the ratio Wt/Q is constant. This constant is called the electromotive force round the circuit.

$$\text{Thus } E = Wt/Q \quad \text{or} \quad EQ = Wt.$$
$$\text{But } Q = it; \text{ hence } Ei = W.$$

This equation enables us to define the unit of electromotive force: for, if $i = 1$, so that the electromagnetic unit of current is traversing the circuit, and $W = 1$, so that one unit of work is being done in unit of time, then $E = 1$. So that the unit electromotive force will cause unit current traversing a circuit to do unit work per unit of time. We can also find the dimensions of the unit of electromotive force on this system, for those of W and i are known: substituting these in the fundamental equations we obtain—

$$[E] = [L]^{3/2} [M]^{1/2} [T]^{-2}.$$

We see from the above that to measure an electromotive force absolutely we have to measure work and current.

The third fundamental unit is that of resistance, and this is defined by means of Ohm's Law, which tells us as the result of experiment that for a given conductor in a given physical state the ratio of the electromotive force to the current is a constant. Thus, if R denote resistance, we have

$$E/i = R \quad \text{or} \quad E = iR.$$

Thus a wire has unit resistance when unit E.M.F. produces unit current; while for the dimensions of R we find, putting in those of E and i,

$$[R] = [L] [T]^{-1}.$$

We have thus obtained absolute definitions (*i.e.* definitions founded on the three fundamental units of length, mass, and time) of an electrical current, electromotive force, and electrical resistance. We can summarise thus:

The electromagnetic unit of current flowing in a wire of unit length bent into the arc of a circle of unit radius produces unit force on a unit magnetic pole placed at the centre of the circle.

The unit electromotive force does unit of work per unit of time when causing unit current to flow round a circuit.

The unit of resistance is the resistance of a circuit in which unit electromotive force produces unit current. Further, it is universally agreed now that the units of length, time, and mass referred to are the centimetre, the second, and the gramme: the above definitions, taking these as the fundamental units, give the absolute C.G.S. electrical units.

It was found, however, that when the attempt was made to realise these units they were not very convenient in practice. The unit of current was somewhat too great, the units of E.M.F. and resistance were far too small. We have thus a second system of practical units to which names have been given:

The Ampere $= \frac{1}{10}$ of the C.G.S. unit of current.
The Volt $= 10^8$ C.G.S. units of electromotive force.
The Ohm $= 10^9$ C.G.S. units of resistance.

Lord Rayleigh's work at Cambridge made it possible to measure to a high degree of accuracy a current in amperes, an electromotive force in volts, and a resistance in ohms.

The introduction of this system of units is due to the labours of the British Association Committee on Standards of Electrical Resistance, whose reports published from 1862 onwards have been of the utmost importance in determining the progress of electricity as an exact science. Lord Kelvin (then Professor Thomson) was the moving spirit in the

establishment of the Committee and the inspirer of much of its work. He had realised more fully than other men from his labours connected with the Atlantic Cable the necessity for definite standards for electrical measurement: and he never wavered from the belief that those standards must be absolute, based on the fundamental units of length, mass, and time.

Electrical resistance is a definite property of a conductor, just as its length or its density, and hence the resistances of various wires had, previous to this time, been adopted as standards. Thus Lenz in 1838 used the resistance of one foot of No. 11 copper wire. Wheatstone in 1843 proposed one foot of copper wire weighing 100 grains. In 1846 Hankel used a length of iron wire, while in 1848 Jacobi issued as a standard a certain length of copper wire known since as Jacobi's standard. When telegraphs were introduced the measurement of resistance became of commercial importance, and in England a mile of No. 16 copper wire was adopted as a unit, in Germany the German mile of No. 8 iron wire was used, and in France the kilometre of iron wire 4 millimetres in diameter. The inconveniences of these various standards in time became apparent; moreover it was realised that metals such as iron and copper varied in resistance with their purity and could not be relied on for uniformity. To remedy this Dr. Werner von Siemens introduced the Siemens unit, the resistance of a column of mercury 100 cm. in length and 1 sq. mm. in section at the temperature of melting ice.

The British Association Committee appointed in 1861 decided in their first report (1862) that a resistance based on the absolute system was the most convenient unit that they could adopt. In determining this they were of the opinion that the unit chosen should combine, as far as was possible, the five following qualities:

(1) The magnitude of the unit should be such as would lend itself to the more usual electrical measurements without requiring the use of extravagantly high numbers of cyphers or of a long series of decimals.

(2) The unit should bear a definite relation to units which may be adopted for the measurement of electrical quantity, current, and electromotive force: or, in other words, it should form part of a complete system for electrical measurements.

(3) The unit of resistance, in common with the other units of the system, should, so far as is possible, bear a definite relation to the unit of work, the great connecting link between all physical measurements.

(4) The unit should be perfectly definite, and should not be liable to require correction or alteration from time to time.

(5) The unit should be reproducible with exactitude in order that, if the original standard were injured, it might be replaced: and also that observers, who may be unable to obtain copies, may be able to manufacture them without serious error.

A system of units in which the unit of resistance satisfied these conditions had been introduced by Weber, and his system was adopted by the Committee. The primary work of the Committee was to construct a material standard and measure its resistance accurately in terms of this unit. When this was done, a standard of resistance having a value determined absolutely was in existence: the task of comparing other resistances with this was a comparatively simple matter.

Weber had already carried through this task and resistance coils had been issued by him having their values stated in absolute units, but it was clearly desirable to check his results, and from this came the celebrated spinning coil

experiments carried out by Maxwell and Fleeming Jenkin at King's College and the first determination of the ohm, the new standard, in absolute measure. The Committee adopted the name Ohm as the name of their material standard, intended, as has been said, to represent as accurately as possible 10^9 C.G.S. units of resistance in electromagnetic measure: if their determination were wrong the ohm would need alteration. Modern practice is somewhat different: the ohm is the name of 10^9 C.G.S. units and does not depend on the accuracy of any one or more experiments. If an experiment has been wrongly performed, the value in ohms of the material resistance, which expresses the result of the experiment, will be erroneous, but the ohm itself is unaffected.

In later years the name 'B.A. unit' was adopted for the standard issued by the Committee: the result of their experiments is expressed by the statement—

$$1 \text{ B.A. unit} = 1 \text{ ohm} = 10^9 \text{ C.G.S. units.}$$

Lord Rayleigh's work, as we shall see shortly, leads to the result that the B.A. unit is about 1·3 per cent. smaller than it was intended to be, or

$$1 \text{ B.A. unit} = \cdot 9867 \text{ ohm} = \cdot 9867 \times 10^9 \text{ C.G.S. units.}$$

In the Committee's experiments a circular coil of wire was made to rotate at a uniform rate about a vertical diameter. The earth's magnetism induces an electric current in the coil, and this current deflects from its equilibrium position a small magnet delicately poised at the centre of the coil. The deflecting force depends on the strength of the induced current, and this again is proportional to the strength of the earth's magnetism, to which the inducing force is due, and inversely proportional to the resistance of the coil. The magnet is pulled back to the meridian

by the earth's magnetism, and the position it ultimately takes up depends on the ratio of these two forces. Since both the forces are proportional to the earth's magnetism, this quantity disappears from the ratio. Thus we get a direct relation between the resistance of the coil and the deflection, and hence are able, by measuring the deflection and the rate at which the coil rotates, to express the resistance absolutely.

But there are various known methods of comparing the resistance of the rotating coil with standards of resistance: hence the resistance of these standards can be measured absolutely.

This rough theory requires, of course, numerous corrections before an exact value can be found: it may serve to indicate generally the process. The results of the Committee were published in 1864.

The Committee did not undertake the absolute determination of a standard of current or of electromotive force. This, however, was done in France and Germany, while other determinations of the standard resistance were made. Chief among these was Rowland's, published in 1878.

In 1880 the meaning of absolute measurements and their fundamental importance were becoming more generally recognised, and when Lord Rayleigh accepted the Cambridge Professorship it was clear that the uncertainties which existed in the absolute values of the fundamental units were too great.

The second volume of Lord Rayleigh's Collected Papers covers the period 1881–1887, and most of the work therein described was carried out at Cambridge, though some of it was only published at a later date.

Those who have interested themselves in fundamental electrical measurements realise most fully the change that was produced by the researches carried on at the Cavendish

Laboratory. An extract from the preface to the volume in which they are described will give the position then.

> Among the Papers here reprinted, several relating to the electric units were written conjointly with Professor Schuster and Mrs. Sidgwick. It may perhaps be well to remind the reader that at the time of these researches the ohm was uncertain to the extent of 4 per cent., and that the silver equivalent then generally accepted differed 2 per cent. from the value arrived at by us.

By means of the Ayrton-Jones apparatus recently erected at the National Physical Laboratory, the electro-chemical equivalent of silver is now known to some few parts in one hundred thousand, and this accuracy is the direct outcome of Lord Rayleigh's work.

Rowland's investigation of the absolute value of the B.A. unit was published in 1878, and from this it appeared that the value found by the B.A. Committee in 1877 was about 1 per cent. too small; other observers had found different values, and it was clear that the uncertainty was considerable, while there was no doubt of the importance of the matter. The original apparatus of the Committee was at the Cavendish Laboratory, and before Lord Rayleigh's appointment to the chair Chrystal had made preparations to repeat the determination though by a different method. One of the new Professor's earliest courses of lectures was on the history of the fundamental Electrical Measurements and very shortly after his inauguration he commenced the experiments. Aided by Schuster and Mrs. Sidgwick he re-erected the old spinning coil apparatus, modifying it to secure higher accuracy. The method involves reading the deflection of a magnet supported at the centre of a coil of wire which is rotating at a uniform speed, and to a first approximation the quantities to be observed are the tangent of the deflection and the speed of rotation. The speed was determined to a

high degree of accuracy by the now well-known stroboscopic method of measurement, and careful steps were taken to reduce the disturbing action of vibrations set up in the box containing the magnet and mirror by mechanical disturbances.

But the main interest of the work lay in the investigation of the outstanding corrections; the result is influenced by the self-inductance of the revolving coil, and Rayleigh's work showed that the Committee's estimate of the amount of this correction was about one-tenth of its true value.

In the end the value found was that—

$$1 \text{ B.A. unit} = \cdot 9893 \times 10^9 \text{ C.G.S. units,}$$

while Rowland's results had given—

$$1 \text{ B.A. unit} = \cdot 9911 \times 10^9 \text{ C.G.S. units,}$$

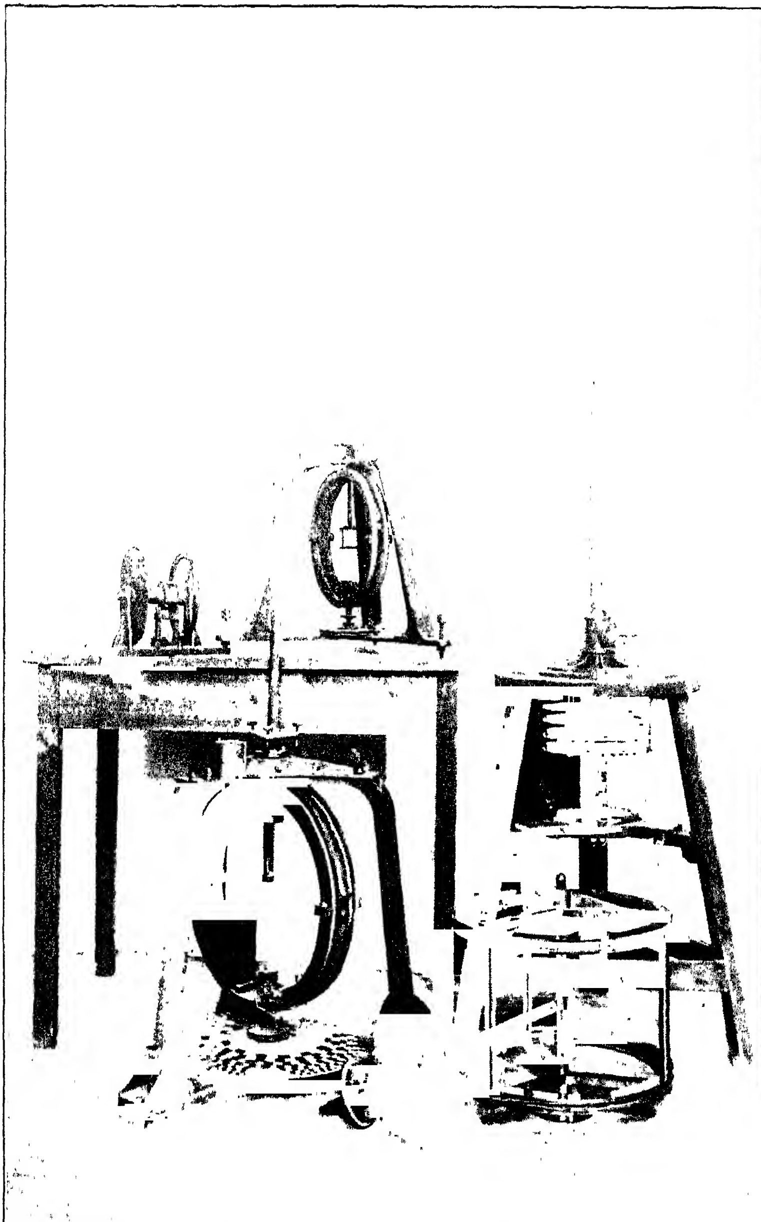
a sufficiently close result to show that the B.A. unit was about 1 per cent. too small.

But the apparatus was not in a sufficiently satisfactory state to give results of the highest accuracy; the frame in which the coil was wound was damaged and it was not possible to make the geometrical measurements as closely as they were required; and so a new apparatus was constructed with the help of experience gained in the first series of experiments. Lord Rayleigh made this second coil and its framework somewhat larger and stiffer than the first, while great care was taken in measuring its dimensions and calculating its constants.

The results were communicated to the Royal Society and appeared in the *Phil. Trans.* for 1882 ('Collected Papers,' Vol. II. p. 38), and the final value is—

$$1 \text{ B.A. unit} = \cdot 98651 \times 10^9 \text{ C.G.S. units.}$$

The difference, three parts in a thousand, between this and the previous result is assigned by Lord Rayleigh mainly



SOME APPARATUS USED IN CLASSICAL RESEARCHES

to uncertainty in the measurement of the coil of the original apparatus.

Next year (*Phil. Trans.* 1883, 'Collected Papers,' Vol. II. p. 94) another paper on the same subject, but by a different method, appeared written by Lord Rayleigh and Mrs. Sidgwick.

Lorenz in 1873 had employed a very simple method for solving the problem. A circular disc rotates about an axis perpendicular to its plane in the magnetic field due to a concentric coil. By balancing the fall of potential between the centre and the edge of the disc against that due to the passage through a resistance of the current producing the field, the value of the resistance is given by the formula $R = n$ M, where R is the resistance, M the coefficient of mutual inductance between the coil and the disc, and n the number of revolutions of the disc per second. Lord Rayleigh realised that the method offered in some respects important advantages. The frame used in the rotating coil experiment and the means for producing uniform rotation were to hand, while two coils wound with great care by Chrystal, and already used by the present writer for a determination of the B.A. unit by another method, were suitable for the experiment.

With regard to the use of these two coils Lord Rayleigh points out that an error in the mean radius would affect his result and mine in the opposite direction, since in his case the sum of the number of turns on the coils enters into the calculation, while I was concerned with their product.

The result[1] of this series of observations gave—

$$1 \text{ B.A. unit} = \cdot 98677 \times 10^9 \text{ C.G.S.}$$

In connexion with this part of his work reference should

[1] Glazebrook's result was—

$$1 \text{ B.A. unit} = \cdot 98665 \times 10^9 \text{ C.G.S.}$$

be made to a paper by Lord Rayleigh in the *Phil. Mag.* Vol. XIV. p. 329, on 'A Comparison of Methods for the Determination of Resistances in Absolute Measure.' The paper indicates in a very clear manner the sources of error in the different methods and the probable reliability of the results.

But these three papers did not complete Lord Rayleigh's work on resistance measurement at the Cavendish Laboratory. For various reasons the resistance of a uniform column of mercury at the temperature of melting ice one square millimetre in section and of some definite length had been adopted as a convenient standard. The legal ohm of the Paris Congress of 1881 was the resistance of such a column 106 centimetres long, and the question naturally arises, what is the length of such a column which has a resistance of one ohm in C.G.S. units? The paper on the specific resistance of mercury (*Phil. Trans.* Vol. 174, 1882, 'Collected Papers,' Vol. II. p. 78) answers this question, and from it we obtain the result that the desired length is 106·21 centimetres.

This result was obtained by comparing the resistance of carefully calibrated tubes containing mercury with the B.A. unit, and from the comparison it appeared that the resistance of a column of mercury 100 cm. long, 1 sq. mm. in area

$$= \cdot 95418 \text{ B.A.U.}$$
$$= \cdot 94149 \text{ ohm,}$$
$$\text{since } 1 \text{ B.A.U.} = \cdot 9867 \text{ ohm.}$$

Hence 1 ohm is the resistance of a column 100/·94149 or 106·21 cm. in length.

Determinations of this important quantity have been made by many other observers. A table of the results for those experiments in which the same resistance coil was measured absolutely and also in terms of mercury follows:—

The Ohm in terms of a Column of Mercury.

Year	Observer	Method	Value of Ohms in centimetres of mercury
1882	Rayleigh[1]	Rotating coil	106·31
1883	Rayleigh[1]	Lorenz	106·27
1884	Mascart	Induced current	106·33
1887	Rowland	Mean of several methods	106·32
1887	Kohlrausch	Damping of magnets	106·32
1882–1888	Glazebrook	Induced currents	106·29
1890	Wuilleumeier	——	106·31
1890	Duncan and Wilkes	Lorenz	106·34
1891	Viriamiu-Jones	Lorenz	106·31

[1] These two results of Lord Rayleigh's experiments have been reduced by using the value ·9534 B.A.U. for the value of the resistance at 0° of a column of mercury 100 cm. in length and 1 sq. mm. in section instead of the value ·9542 used by Lord Rayleigh himself. See p. 70.

Lord Rayleigh had thus determined one of the three fundamental units of electrical measurement and had expressed his result in a reproducible form; and it remained to determine with equal accuracy the other two, the ampere and the volt. The three are, of course, connected by Ohm's law; it is sufficient to determine the ampere, and from a knowledge of it and of the ohm the volt can be deduced.

The electro-chemical action of an electric current had from the time of Faraday afforded a method of measuring it. The ampere can be defined in terms of the quantity of silver it deposits in a given time, and it was this quantity which Lord Rayleigh undertook to measure. A coil was suspended from one arm of a balance with its plane horizontal and midway between two fixed coaxial coils of larger radius. The electro-dynamic attraction between the suspended and fixed coils when carrying the same current can

be balanced by weights in the opposite pan; it can also be calculated in terms of the current and the dimensions of the two coils; the current is thus measured absolutely in terms of the weight and the dimensions of the coils.

If the same current also traverses a solution of nitrate of silver in a platinum bowl suitably arranged, it can also be measured in terms of the weight of silver it deposits, and thus we can express a current whose value is known in absolute units in terms of the silver deposited.

The experiment had been made with great care by Professor Mascart, who had found that a current of one ampere deposits per second ·0011156 grammes of silver, while Kohlrausch had given as the result of his observations the figure ·0011183 grammes.

As a standard of electromotive force the Clark cell was chosen and the E.M.F. of the cell was measured by balancing it against the fall of potential between the ends of a wire of known resistance carrying a current of one ampere.

The work was again carried out in conjunction with Mrs. H. Sidgwick, and published in the *Phil. Trans.* for 1884 ('Collected Papers,' Vol. II. p. 278) under the title 'On the Electro-chemical Equivalent of Silver and on the Absolute Electromotive Force of Clark Cells.'

The two fixed coils belonged to an electro-dynamometer constructed by the Electrical Standards Committee of the British Association, and had a mean radius of about 25 cm. The suspended coil was a flat coil of mean radius about 10 cm. and its weight was about 575 grammes.

The mean radius of this small coil was not found by measurement but was compared by an electrical method with that of the large coils. The paper contains an important investigation into the conditions necessary for obtaining uniform results in a silver voltameter and the precautions to be observed in setting up a standard cell.

It also describes the **H** form of Clark cell now uniformly adopted for standard work.

In the end the following values were found:—

Number of grammes of silver deposited per second by a current of 1 ampere = ·00111794.

E.M.F. of Clark cell at 15°C. = 1·4345 volts.

The value for the silver equivalent, it will be observed, differs considerably from that found by Mascart, but is nearly the same as—actually 4 parts in 10,000 less than—that given by Kohlrausch. Professor Mascart a few months before his death gave to the present writer an explanation of the difference; it appears that he used in his determination of the dimensions of his coils a metre standard which he had every reason to believe was correct. A comparison, however, at a date long subsequent to 1884 proved this to be a mistake, and when the error was corrected a value closely approaching Lord Rayleigh's was the result.

The Tables which follow give the principal values found for these two fundamental electrical units by other experimenters. Values for the E.M.F. of the Weston cell which has recently taken the place of the Clark cell are also included.

Values obtained for the Electro-chemical Equivalent of Silver

Year		Milligrammes
1884	Mascart	1·11561[1]
1884	Kohlrausch	1·1183
1884	Rayleigh and Sidgwick . . .	1·1179
1890	Pellat and Potier	1·1192
1899	Kable	1·1183
1903	Pellat and Leduc	1·1195
1904	V. Dijk and Knust	1·1182
1906	Guthe	1·1182
1907	Smith, Mather, and Lowry . .	1·1183
1908	Janet, Laporte, and De la Gorce .	1·1182

[1] In this determination, a numerical error was made. When corrected for this, the value becomes 1·118.

E.M.F. of Clark Cell at 15°C.

Year		Volts
1872	Clark	1·4378
1884	Rayleigh and Sidgwick . . .	1·4345
1896	Kable	1·4322
1899	Carbart and Guthe	1·4333
1905	Guthe	1·433
1907	Ayrton, Mather, and Smith . .	1·4323
	Indirect Determinations:—	
1884	V. Ettinghausen	1·4335
1885	Rayleigh	1·435
1892	Glazebrook and Skinner . . .	1·434
1904	Trotter	1·4329

E.M.F. of Weston Normal Cell at 20°C.

Year		Volts
1906	Guthe (National Bureau of Standards)	1·0185
1907	Ayrton, Mather, and Smith (National Physical Laboratory) . . .	1·0182
1908	Janet, Laporte, and Jouast (Laboratoire Central d'Électricité) . . .	1·0187
1908	Lippmann and Guillet . . .	1·0182
1908	Pellat	1·0184

Meanwhile the necessity for legislation with regard to Electrical Units became apparent. In 1890 the Board of Trade appointed a Committee to consider the question; the first report of this Committee, issued in 1891, is based almost entirely on the results of Lord Rayleigh's Cambridge work, and these figures, with but slight alteration,[1]

[1] The one definition in which Lord Rayleigh's figures are materially departed from is in the length of the column of mercury which has a

were adopted in the British Order in Council of August 23, 1894, legalising certain new demonstrations of standards for the measurement of Electricity. The figures given in the Board of Trade Report were adopted at the International Electrical Congress at Chicago in 1892, and have since formed the foundation of legislation throughout the world.

But this great series of electrical papers does not at all exhaust Lord Rayleigh's activities while at Cambridge.

Another *Phil. Trans.* paper of very real importance is one published in 1885 on 'The Constant of Magnetic Rotation of Light in Bisulphide of Carbon' ('Collected Papers,' Vol. II. p. 360). This appeared, it is true, after the Cambridge period, but the experiments were all carried out at the Cavendish Laboratory. The second volume of the 'Collected Papers' contains some fifty papers, all falling within the five years during which he held the Professorship.

These writings are all marked by the same characteristics: perfect clearness and lucidity, a firm grasp on the essentials of the problem, and a neglect of the unimportant. The apparatus throughout was rough and ready, except where nicety of workmanship or skill in construction was needed to obtain the result; but the methods of the experiments, the possible sources of error, and the conditions necessary to success were thought out in advance and every precaution taken to secure a high accuracy and a definite result.

Those of us who were privileged to work with him in those days, to learn of him how truth was to be sought and the difficulties of physical science unravelled, owe him a very real debt. His example and his work have inspired his successors in the Laboratory, and the great harvest of

resistance of 1 ohm. This was taken as 106·3 instead of 106·21 as given by his experiments. More recent experiments have given ·9534 B.A.U. as the value of the resistance of 100 cm. of mercury, and this value was taken by the Committee.

discoveries which has in recent years been reaped in Cambridge springs from seed sown by him.

Nor was the time barren in results attained by other workers: the school was young and the numbers of research students could not be large, but we were enthusiastic and some at least of the work must be mentioned. When Lord Rayleigh was appointed, Fleming was just completing that examination into the relative resistances of the old B.A. units which was needed for much of Lord Rayleigh's work, tracing out on a chart the values of the coils which served until the year 1894, when the Board of Trade Ohm was established by Order in Council as the resistance standard of the country. Schuster's name has already been mentioned as collaborating with Rayleigh in the work on the absolute measurement of resistance and in the theoretical side of this work. W. D. Niven also assisted.

The British Association Committee on the Measurement of the Lunar Disturbance of Gravity was active, and in 1880–81 G. H. and H. Darwin set up in one of the rooms of the Laboratory their delicate bipolar pendulum in the hope of detecting and measuring its amount; the work went on for some time, and, while the results were both interesting and important, the temperature conditions on the surface of the Earth were such as to mask the effect sought for. It has been reserved for Hecker in his well at Potsdam to measure in the course of the last few years the tides in the solid earth for which the Darwins, inspired by Kelvin, were looking nearly thirty years ago.

Shaw's meteorological investigations began in 1880; a report on evaporimeters was published during the period under discussion, and much of the work for the important comparison of various forms of hygrometers, printed in the *Phil. Trans.* for 1888, was done while Lord Rayleigh was Professor. During the same period too he carried out, with

Mrs. Shaw's assistance, the experiments described in the paper 'On the Atomic Weights of Silver and Copper' (*Phil. Mag.* 1887).

J. J. Thomson's first paper 'On the Determination of the Number of Electrostatic Units in the Electromagnetic Unit of Electricity' appeared in the *Phil. Trans.* for 1883, leading to the result $2·963 \times 10^{10}$ cm. / sec. as the value of 'v,' a result somewhat modified in a second paper written in conjunction with G. F. C. Searle (*Phil. Trans.* 1890), which gives the value $2·9955 \times 10^{10}$ cm. / sec. The first of these papers was the earliest of the long series which the Cavendish Professor has published as the result of researches carried out in the Laboratory.

Glazebrook's paper 'On the Value of the British Association Unit of Resistance' was printed in the *Phil. Trans.* for 1883, the same volume which contains Lord Rayleigh's Lorenz paper.

The experiments were carried out with the assistance of Messrs. Dodds and Sargant, and led to the result already given that—

$$1 \text{ B.A.U.} = ·98665 \times 10^9 \text{ C.G.S. units};$$

while in a somewhat later paper (*Phil. Trans.* 1888) Glazebrook and Fitzpatrick obtained the result that—

$$\text{Resistance of } 100 \text{ cm. mercury} = ·95352 \text{ B.A.U.}$$

During the earlier part of Lord Rayleigh's Professorship, Glazebrook had been investigating the 'Reflexion of Light from the Surface of a Uniaxial Crystal' (*Phil. Trans.* 1882), while throughout the time work on the resistance standards of the B.A. was going on in connexion with the Electrical Standards Committee.

Another paper which should be mentioned is one by

J. C. McConnel, of Clare, who died at an early age at Davos, on 'The Form of the Wave Surface in Quartz.'

This incomplete list of researches carried through and investigations published is perhaps enough to indicate the activities of the Laboratory during the years 1880–1884, and to show the influence the Professor had in helping the junior members of the staff and others who carried out work under his supervision and guidance.

It is impossible for me, closely connected as I was with the Laboratory and its growth, to estimate the value of these years. To myself they were perhaps the happiest I ever spent. This brief account can give but an imperfect picture of the work and workers of the Rayleigh period.

CHAPTER IV

SURVEY OF THE LAST TWENTY-FIVE YEARS

I HAVE been asked by the editors of this volume to give an account of the history of the Cavendish Laboratory and the researches made in it during the time I have held the professorship. I find on revising what I have written that I have been sadly garrulous and discursive; the fact is I have found my task very difficult as well as very pleasant. The reference to old records and letters has revived many half-forgotten incidents and recalled so many pleasant recollections that this chapter has far exceeded the limits I contemplated when I began to write.

My connexion with the Cavendish Laboratory began in 1880, immediately after I had taken my degree, for although I had come up to Cambridge in 1876 the preparation for the Mathematical Tripos had left me no leisure to do any experimental work. At that time there was only one examination for the Mathematical Tripos, and candidates were expected to be acquainted with the whole of the wide range of pure and applied mathematics included in the examination. The range of reading was much wider and there was much less specialisation under this system than under the one which succeeded it, when the more elementary subjects were grouped into one part of the Tripos, and candidates were allowed to select a small number of the more advanced subjects for the other part. There is naturally, when we look back on our early days, a kind of

glamour over many of our experiences, and they look more attractive than they did at the time or than they would to an unbiased judgment; but examinations are about the last things to which sentiment would cling, and I do not think it is prejudice which makes me prefer the system in vogue when I took my degree to those which succeeded it. I confess I am glad that I came under the older scheme, for probably I read much more widely in pure mathematics than I should have done if I had taken my degree a few years later. I have found this reading of great value: not the least valuable part of even a superficial acquaintance with the higher parts of pure mathematics is that it prevents one from being deterred by the formidable aspect which pure mathematicians, by their nomenclature, delight to impart to the simplest and most evident theorems.

The weak part of the old scheme was the order of merit, which compelled us to devote a large portion of our time to acquiring the art of getting in twenty minutes results which would otherwise perhaps have taken three-quarters of an hour. The physical subjects too were considered much too exclusively from the purely mathematical side, though in this respect it was no worse than its successors. If we could have spent some of the time we devoted to acquiring a useless dexterity in dealing with artificial problems to working in the Laboratory and gaining a first-hand acquaintance with physical phenomena, the old system would, in my opinion, have left little to be desired.

Fortunately before coming to Cambridge I had had at the Owens College, Manchester, an opportunity of working in a physical laboratory under Professor Balfour Stewart, and had even done a small piece of research on Contact Electricity between non-conductors. When I was at the Owens College in the middle seventies physical laboratories were not so crowded as they are nowadays, and the students

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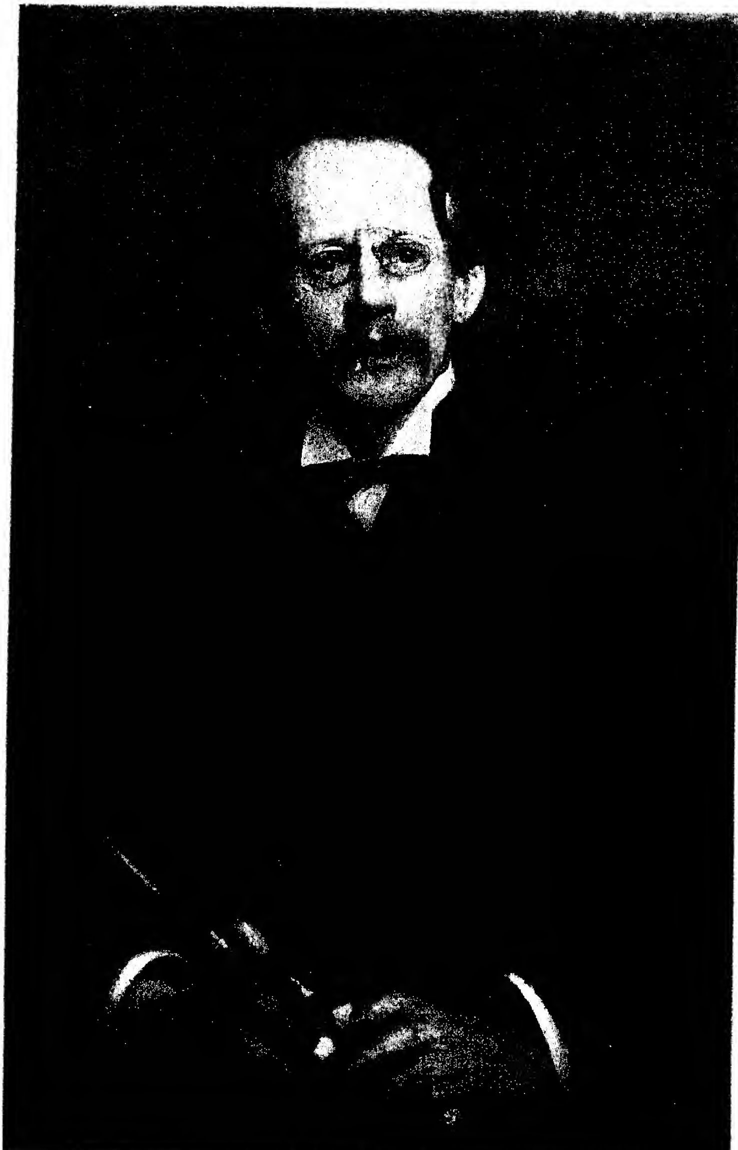


JOSEPH JOHN THOMSON.

CAVENDISH PROFESSOR 1884.

After the portrait by Arthur Hacker, A.R.A.

were allowed to work more independently than is possible where the numbers are large. We were allowed considerable latitude in the choice of the experiments we made, we set up our own apparatus and spent as much time as we pleased in following any points of interest that turned up in the course of our work; the work was thus from the first more interesting and more educational than under the highly organised systems of instruction which a large number of students necessitates.

Professor Balfour Stewart, in addition to being a most excellent teacher, was enthusiastic about research and succeeded in imparting the same spirit to his pupils. I remember that one day, when I had only been a short time in the Laboratory, he was talking about sun-spots and he said that he had a large number of observations which he thought might throw some light on the connexion between sun-spots and terrestrial phenomena, but that he had not the time to reduce them. I ventured to say that if I could be of any help I should be glad to do what I could, and he gave me a number of observations to reduce. Though the work I did was purely arithmetical, I found it very interesting, and enjoyed the feeling that I was taking some part in real science, a feeling which is not given by the repetition of a laboratory experiment.

Balfour Stewart was much interested in the question as to whether there is any change in weight when substances combine chemically. When I was helping him with some experiments on the combination of mercury and iodine these substances combined with such violence that the flask they were in exploded in my face and I nearly lost the use of both eyes.

Whilst I was in the laboratory at Manchester, Schuster was working at the spectroscope; he gave a course of lectures on Maxwell's 'Electricity and Magnetism,' which

I attended, and Poynting, another old student, returned to it for a short time after taking his degree at Cambridge.

I left Manchester and came into residence in Cambridge in 1876. The only original investigations I completed before taking my degree in January 1880 were two short papers on Pure Mathematics, which were published in the *Messenger of Mathematics.* Most of my time when I was an undergraduate was spent in reading for the Tripos and in attending lectures. The lectures of the mathematical professors in those days were not attended by many undergraduates; at one course of lectures given by Professor Cayley I was the only undergraduate present, and as the audience only amounted to three (Dr. Glaisher, Mr. R. T. Wright, and myself), Professor Cayley adopted a somewhat unusual method of lecturing: he sat at the head of the table, and worked away, writing with pen and ink on large-sized paper in front of him; as the seats next to the Professor were occupied by my seniors, I only saw the writing upside down; this, as might be imagined, made note-taking somewhat difficult. I should, however, be very sorry to have missed these lectures: it was a most valuable and educational experience to see Cayley solve a problem, using apparently the first method that came to his mind, a process which often led to analytical expressions which seemed hopelessly complicated and uncouth. Cayley, however, never seemed deterred, but went uninterruptedly on, and in a few lines he had changed the shapeless mass of symbols into beautifully symmetrical expressions and the problem was solved. As a lesson on teaching one not to be frightened by a crowd of symbols it was most valuable.

Professor Adams, whose lectures I also attended, never seemed to have any complicated collection of symbols to deal with; he, like Kirchhoff, carried the feelings of an artist into his mathematics, and a demonstration had to be

elegant as well as sound before he was satisfied. His lectures were wonderfully clear and were read from beautifully written manuscripts, which he brought into the lecture-room in small calico bags.

Of all the lectures I attended I think I enjoyed Sir George Stokes's the most; they were much more physical than the others and were illustrated by experiments which succeeded with a precision which I have often envied since when I have had to do lecture experiments myself. Stokes had, I think, an unrivalled power of explaining the essentials of a physical problem in a clear and simple way, very often without the use of mathematical symbols. In fact, he practised Bacon's aphorism that the physics ought to precede the mathematics, and often had developed the main features of the problem from physical principles before he began the analytical investigation. His interest in his subject sometimes made him forget that the canonical time for a lecture is one hour; but I do not remember that he ever went on so long as he did on one occasion many years after, when he was over eighty years of age. One afternoon at half-past two I met a batch of my students coming into the Laboratory and looking very hungry; I asked them what they had been doing, and they said they had just come from Sir George Stokes's lecture, which had commenced at twelve.

I was much interested in, and derived great benefit from, the lectures on mathematical physics given in Trinity College by Sir W. D. Niven, and those on pure mathematics by Dr. Glaisher; indeed, at one time Dr. Glaisher made me quite enthusiastic about elliptic functions. Some of the College lectures were, I must acknowledge, the reverse of interesting; one lecturer adopted a method I have never seen before or since. He hardly spoke a word, but wrote steadily on the blackboard; when he had

filled it he told us to copy that down, and waited in silence until those who obeyed him had done so; then he cleaned the board and began again. Like nearly everyone else, I 'coached' with Dr. Routh during the time I was preparing for the Tripos. Routh's teaching was not the least like what is ordinarily understood by coaching, it was really an admirably arranged series of exceedingly clear lectures given to a much larger audience than most of the lectures of the University professors or College lecturers.

My connexion with the Cavendish Laboratory began in 1880, immediately after taking my degree. Lord Rayleigh had just been elected to the professorship. My first piece of work was an attempt to detect the existence of some effects which I thought ought to be produced by Maxwell's 'displacement currents' in dielectrics. The results I obtained, however, were not sufficiently definite to allow of any positive conclusions being drawn from them, and I turned my attention to some effects produced by the electrostatic capacity of coils, and subsequently, at Lord Rayleigh's suggestion, to the determination of the ratio of the electrostatic to the electromagnetic unit of electrical charge. I also worked at Mathematical Physics, on such questions as the application of Lagrange's generalised equations to problems in physics and chemistry. This formed the subject of my Fellowship Dissertation at Trinity College, and was subsequently published in an expanded form in my 'Applications of Dynamics to Physics and Chemistry.' Other subjects at which I worked were the Electromagnetic Theory of Light, the Properties of Moving Electrified Charges, Vortex Motion, and the theory of Electric Discharge through Gases. I did some private coaching (my first pupils were Sir Eldon Gorst and Mr. Austen Chamberlain), I lectured at what was then Cavendish College, and was afterwards appointed Assistant Mathematical Lecturer

at Trinity College and University Lecturer in Applied Mathematics.

There was great activity at the Laboratory during Lord Rayleigh's tenure of the professorship. Courses for teaching practical physics were organised by Glazebrook and Shaw, and a scheme of lectures in physics for students preparing for the Natural Sciences Tripos was arranged. It was at this time that the classical determinations by Lord Rayleigh of the absolute values of the fundamental units of electricity were carried out. The number of those doing original research at the Laboratory continually increased: Mrs. Sidgwick, Schuster, Glazebrook, Shaw, Poynting, E. B. Sargant, McConnel, Fleming, Hart, Miss Harland, J. H. Middleton, worked in the Laboratory whilst Lord Rayleigh was professor. The chief mechanical assistant was Mr. George Gordon, who rendered indispensable assistance to those engaged in research. At the end of 1884 Lord Rayleigh resigned the professorship, and to my great surprise, and, I think, to that of the rest of the University, I was chosen to succeed him. I remember I was told at the time that one well-known college tutor had expressed the opinion that things had come to a pretty pass in the University when mere boys were made professors.

My want of experience was made less harmful than it would otherwise have been by the kindness of Glazebrook and Shaw, who continued to take charge of the classes in Practical Physics which had been organised by them; in conducting these classes they had the assistance of McConnel and Randell as Assistant Demonstrators. It is worthy of note that every member of the teaching staff at this time had taken the Mathematical Tripos, while only two of them, Shaw and Randell, had taken the Natural Sciences Tripos in addition.

One of my first duties was to appoint an assistant to

superintend the workshops, as Gordon left to be private assistant to Lord Rayleigh. Everyone acquainted with laboratories knows how much the success of the laboratory depends on the efficiency of the assistant and what a variety of qualifications he must possess. He must be a skilled workman, a man of strong character, for he has to maintain discipline among the younger assistants; he requires, too, the tact of a diplomatist to reconcile the claims on the workshop of those engaged in teaching and those working at research. I was fortunate in finding in Mr. D. S. Sinclair one who combined all these qualities, and who rendered excellent service to the Laboratory up to his departure in 1887, to take up an appointment in India. He was succeeded by Mr. A. T. Bartlett, who was with us until 1892, when he left to take up electrical engineering. From 1892 to 1899 we had the very efficient assistance of Mr. W. G. Pye, who left us in order to start in business as a scientific instrument maker. Our present Chief Assistant, Mr. F. Lincoln, was promoted to this position in 1899, but he had been a long time before that in the Laboratory, having come to us as an exceedingly small boy in 1892.

The chief lecture assistant, Mr. W. H. Hayles, was appointed by me in the first term of my professorship, while another assistant, Mr. James Rolfe, can boast a longer tenure of office than I can myself, as he was appointed in Lord Rayleigh's time. My private assistant, Mr. E. Everett, has been with me for more than twenty years and has given me most valuable and able assistance in all my investigations.

Immediately after my election to the professorship I began, in collaboration with my old friend Richard Threlfall, one of the most skilful experimenters I have ever met, some investigations on the discharge of electricity through

gases, and since then I think there has never been a time at which I have not had some experiments in hand on this subject. I was led to investigations on this subject by having come to the conclusion that whenever a gas conducts electricity some of its molecules must have been split up, and that it is the molecules which have been thus modified which impart electrical conductivity to the gas; in short, that a gas in which all the molecules are in the normal state must be a perfect insulator. My idea at that time was that some of the molecules were split up into two atoms, one of which was positively, the other negatively, electrified, and my first experiments were intended to test this idea. It was not until 1897 that I discovered that the decomposition of the molecules was of quite a different type from ordinary atomic dissociation; then I found that one of the bodies into which the molecules split up, the one carrying the negative charge, is something totally different from an atom and is indeed smaller in mass than one-thousandth part of the smallest atom known.

Towards the end of my first year of office J. C. McConnel, Fellow of Clare College, a man with a singularly acute and original mind, and who had done valuable work in optics whilst at the Laboratory, left Cambridge to take part in a large business in Manchester with which his family were connected. Unfortunately his health broke down shortly afterwards and he had to spend most of the rest of his life in Switzerland, where he made valuable observations on the properties of ice. He died in 1887. There are in the Laboratory some interesting pieces of optical apparatus which belonged to him, and which, after his death, were presented by his family to the Laboratory.

Threlfall succeeded McConnel as Assistant Demonstrator, and he, too, had a somewhat brief tenure of office, as he was shortly afterwards appointed to the Professorship of

Physics in the University of Sydney, N.S.W. It is characteristic of the man that, though there was no laboratory in the University when he was appointed, before he left he had one of the best physical laboratories in the world, and that he had got the money out of the Government and not from private individuals.

Very soon after I became professor I was fortunate enough to be able to persuade H. F. Newall, now Professor of Astro-Physics in the University, to come and help in the work of the Laboratory, and for some time we worked at original investigations together. Later, when Shaw became tutor of Emmanuel, Newall took his place as Demonstrator and took charge of the teaching of practical physics to students preparing for Part I. of the Natural Sciences Tripos, by far the largest class in the Laboratory.

The students working at research during the first two or three years of my professorship were Threlfall, Newall, Cassie, Wilberforce (who was Demonstrator from 1887 to 1901, when he left to take the Professorship of Physics at the University of Liverpool), Chree, now Superintendent of Kew Observatory; and Callendar, who was elected to a Fellowship at Trinity College for Natural Science after having taken first classes in both Mathematics and Classics, and who began in the Laboratory those researches on the effect of temperature on the resistance of metals which have revolutionised thermometry. Besides his work on physics, Callendar found time to invent a new species of shorthand, which to my mind was much easier to read than the ordinary kind. He was Assistant Demonstrator for a short time, then Professor of Physics at the Royal Holloway College, then at Montreal, and now at the Imperial College of Science. Olearski and Natanson, who worked at this time, were the first of a long series of students from foreign Universities who have come to work at the Laboratory.

The Laboratory received, in 1887, the scientific books and the notebooks and papers of Clerk-Maxwell. These memorials of the first Professor and designer of the Laboratory are not only interesting from their associations, but they form the nucleus of a departmental library which has received important donations from Pembroke College and from the executors of the late Professor Newton.

This is not the only bequest we owe to Maxwell, for the Clerk-Maxwell Studentship was founded in 1890 from funds bequeathed to the University by Mrs. Clerk-Maxwell in 1887. The studentship was established for the promotion of research in Experimental Physics, and the holder is elected for the promise he shows for research and not by any examination. Studentships of this kind are of great value: they give an opportunity to promising students to complete within the three years for which they hold the studentship important scientific work, and to gain at the end of their tenure a scientific reputation which gives them every prospect of obtaining a scientific appointment and justifies them in devoting themselves to science. Many of these might otherwise have been obliged by lack of funds to take remunerative work, such as teaching in a school, immediately after taking their degree, and would have had no opportunity of developing their powers of research, and their services would have been lost to science.

The first Clerk-Maxwell Student was W. Cassie, who was Professor of Physics at the Royal Holloway College from 1894 until his death in 1906. I am glad to have an opportunity of putting on record an instance of the conscientiousness of Professor Cassie. His work during the last few months of his tenure of the studentship was interrupted to some extent by causes beyond his control, and though he was able to do a considerable amount of work, it fell short of the high standard he had

fixed in his own mind as incumbent on the holder of the scholarship. He insisted on giving the stipend he received for these months to the Laboratory for the purchase of apparatus for research; at the same time he asked that his name should not be disclosed as the donor. I think that, now he has passed away, I am justified in putting on record this act of self-sacrifice on the part of a man who was far from rich and whose gift represented a large part of his income. His successors in the studentship have been J. E. Townsend, C. T. R. Wilson, W. C. D. Whetham, H. A. Wilson, O. W. Richardson, and F. Horton.

A scheme which occupied a good deal of my attention in the early years of the professorship was an attempt to found a new Tripos for students of Physics, Chemistry, and Engineering. The examination for this necessarily included a good deal of mathematics, and it was rejected by the Senate largely on the ground that it would overlap the Mathematical Tripos. The problem which it was hoped the proposed Tripos would solve—that of bringing into closer relationship the study of physics and mathematics—has not yet received a complete solution. I feel very strongly that most of the students in the Laboratory would benefit if they studied mathematics more thoroughly than they do at present, while those who are studying applied mathematics would be much better equipped for research in that science if they came into touch with the actual phenomena in the Laboratory.

In 1887 a scientific discovery of the first importance was made, which appealed especially to everyone connected with the Cavendish Laboratory. I mean the detection by Hertz of Electrical Waves. The existence of these waves is a necessary consequence of Maxwell's Electromagnetic Theory, but though numerous attempts to detect them had been made, some of them at the Cavendish Laboratory,

these had all failed owing to the want of a sufficiently delicate instrument to detect their presence. Hertz, by observing the variations in intensity of the minute sparks which pass between conductors brought nearly into contact when electrical waves pass over them, was able to demonstrate the existence of these waves, and thus prove the truth of Maxwell's Electromagnetic Theory.

To the younger school of mathematical physicists in Cambridge who had been brought up on Maxwell's 'Electricity and Magnetism,' the generality and, in essentials, the simplicity of Maxwell's theory had for some time carried conviction, and by us Hertz's discovery was received with the welcome which might be expected from those who found their most cherished convictions confirmed by a series of most beautiful investigations: it confirmed instead of revolutionising our ideas. On the Continent, however, where Maxwell's theory had, for the most part, met with but little support, Hertz's discovery created a revolution in the study of Electromagnetism.

I well remember the enthusiasm of the undergraduates when I repeated Hertz's experiments in my lectures on Electricity and Magnetism in 1888. The enthusiasm spread to all the workers in the Laboratory, and soon experiments on electric waves were going on all over the building.

Many changes took place in the teaching staff of the Laboratory about this period (1887–90); when Threlfall left Cambridge for Australia in 1887, Newall succeeded him as Assistant Demonstrator. The death in 1888 of J. H. Randell, Fellow of Pembroke College, took from the Laboratory a skilful and experienced teacher, who for five years had rendered most valuable services; he was succeeded by H. L. Callendar. In 1887 W. N. Shaw, who with Glazebrook had organised the teaching of Practical Physics in the Laboratory, and had with Glazebrook

directed the teaching of this subject for seven years, accepted the Tutorship at Emmanuel College. His new duties made too great demands on his time to allow of his continuing to act as Demonstrator, involving, as this did, attendance at the Laboratory for practically the whole of every alternate day. He was prevailed upon, however, to continue the lectures on Physics which he had undertaken for many years, and continued to lecture every term until he left Cambridge in 1900 to become Superintendent of the Meteorological Council. His place as Demonstrator was taken by Newall, who had the charge of the largest class in Practical Physics from this time until he left the Laboratory to take charge of the large telescope presented to the University by his father. The vacancies caused by his promotion and the departure from Cambridge of Callendar were filled by the appointments of L. R. Wilberforce (now Professor of Experimental Physics in the University of Liverpool) and T. C. Fitzpatrick (now President of Queens' College).

In 1888 we introduced courses in Practical Physics for medical students who intended to be candidates for the first M.B. examination. Glazebrook had for many years lectured to these students; these lectures were now supplemented by practical work, the teaching of which was put under the charge of Fitzpatrick, who has had the control of it ever since. When Glazebrook left Cambridge in 1890, the lectures to medical students were given by Capstick, and, when he became Junior Bursar at Trinity, by Wilberforce and Fitzpatrick. When Wilberforce left Cambridge in 1900, Fitzpatrick undertook the lectures as well as the demonstrations to medical students, and since then has had the entire control of this part of the work of the Laboratory.

We have often been hard put to it to find room for the

Medical Demonstrations. They were at first given in the Laboratory; then the attendance grew so large that it was impossible to accommodate the students, and we had to look out for a new home for these classes. At last we found one in a tin shed on the south-east corner of the New Museum site which had been used as a temporary dissecting room, and vacated when the Laboratory of Anatomy and Physiology was completed. Our demonstrations remained in this building until 1896, when they were transferred to the southern extension of the Laboratory which was opened in that year. The big room in this building accommodates a very large class, a matter of great importance when, as in the case of medical students, the time-table is so crowded that there are only a small number of hours per week when it is possible for them to attend, and all the classes have to be taken at these times.

In 1891 Glazebrook was appointed Senior Bursar of Trinity College, and the onerous duties of his new office compelled him to give up the demonstratorship which he had held since 1880, when he and Shaw organised the teaching of Practical Physics in the Laboratory. He continued, however, to lecture at the Laboratory until he left Cambridge to become the Principal of University College, Liverpool, now the University of Liverpool. An additional demonstratorship, the stipend of which was paid wholly by Laboratory fees, was founded in 1891. S. Skinner, R. S. Cole, C. E. Ashford, W. C. D. Whetham, and G. F. C. Searle became demonstrators about this time. Searle is still a demonstrator, and has had for many years charge of the very large class in practical physics intended for those students who are taking Part I. of the Natural Sciences Tripos.

I have adopted at the Laboratory a scheme by which

attendance at the Laboratory for teaching purposes is only required from the demonstrators on three days a week. The main object of this scheme is to leave the demonstrators three clear days a week for research, so that a young demonstrator may not be overwhelmed with teaching, but has opportunities for doing original work and acquiring distinction as a physicist. I still, after a long experience, think the scheme a good one, though it is sometimes interfered with when the demonstrator through marriage or some other cause, requires a larger income than the salary of a demonstrator, and has to devote the days on which he does not demonstrate to remunerative work instead of research.

In the early nineties the Cavendish Physical Society was founded. The primary object of the society is the discussion of recently published papers in physics. Not infrequently, however, some one working at research in the Laboratory gives to the society an account of the results he has obtained before he publishes them. This is often very helpful, as it enables the student to see which are the points which he has not made clear to the audience, so that when he rewrites the paper for publication he knows the parts which require further explanation.

The society, besides helping to keep workers in the Laboratory abreast with recent work in physics, gives an opportunity to them of acquiring practice in lecturing, for the majority of the reports on papers are given by the students themselves. The discussions which follow the reading of the report help to clear up difficulties and often suggest subjects for further investigation. The meetings of the society, which are preceded by tea, are held fortnightly in term time.

In 1895 a change was made in the regulations of the

University which had a great influence upon the fortunes of the Laboratory. Under the new regulations graduates of other Universities, whether at home or abroad, and in exceptional cases students who are not graduates, can enter the University for purposes of research or advanced study, provided they satisfy the Degree Committee of the Board of Studies that they are qualified to do research and that the investigation they contemplate is one which can be profitably carried out in the Laboratory in which they propose to work. They are entitled to the B.A. degree and certificates of research work if after two years' residence they submit to the Board a thesis which in the opinion of the Board is 'of distinction as a record of original research,' while if they submit after only one year's residence a thesis on which this verdict has been passed, they are entitled to a Certificate of Research, but not to the B.A. degree until they have completed another year's residence.

In the October term after these regulations were passed, three Research students came to the Laboratory, Rutherford from New Zealand, Townsend from Trinity College, Dublin, and McClelland from Galway; all three are now Fellows of the Royal Society, all three hold important Professorships of Physics, and Rutherford has been awarded a Nobel Prize. These proved to be the distinguished forerunners of a very numerous band of students from practically every important university in Europe, Asia, Africa, and America, and since that time the Research student has been one of the most characteristic features of the Laboratory, giving to it quite a cosmopolitan tone. Mingled with Cambridge students who are staying on to do research after taking their degree, we have graduates of most universities in Great Britain and her dominions, many of them holders of the '1851' Exhibitions, graduates from the United States, generally one or more Professors of American Universities who are spending

their 'Sabbatical year' in research, and graduates from German, French, Russian, and Polish Universities. The advantage gained by our students from this communion of men of widely different training, points of view and temperament can, I think, hardly be over-estimated. In their discussions they become familiar with the points of view of many different schools of scientific thought, leading to a better, more intelligent and more sympathetic apprecia-tion of work done in other countries; they gain catholicity of view not merely on scientific but on political and social questions. If the morning papers contain an account of the occurrence of some striking social or political event in our Colonies or abroad, it is generally mentioned when we meet for tea in the afternoon, and very frequently we find that there is some one among us from the country where the event happened who is able to throw fresh light upon it and make it seem far more vivid than would the reading of any number of telegrams. I have, for example, when a presidential election was taking place in the United States, listened to Republicans and Democrats fighting their battles over again, and have felt that I learnt far more about American politics by listening to their discussions than by reading columns from special correspondents.

Rutherford began at the Laboratory by working at wireless telegraphy, using a detector which he had invented before leaving New Zealand. He soon gave us the record at the time for long-distance telegraphy by signalling from the Laboratory to his rooms about three-quarters of a mile away. Townsend, who had entered the Laboratory almost at the same time, made some very interesting investigations on the magnetic properties of iron when in combination; he showed that the magnetism of the same mass of iron was not the same in the ferrous as in the ferric salts, while in salts like the ferricyanides iron was hardly magnetic at all.

It was fortunate that the new University regulations, which caused a large influx of students to the Laboratory bent upon original research, came just at the time when the discovery of the Röntgen rays gave us a very powerful method of investigating the phenomena attending the discharge of electricity through gases. A great many researches had already been made in the Laboratory on this subject, and our studies had led us to see the fundamental importance of certain investigations which were almost impracticable with the means then at our command of producing electrical conductivity in gases. The Röntgen rays gave us a means of producing this conductivity far more amenable to the purposes of investigation than those hitherto available, and made many investigations possible which had been almost hopelessly difficult before.

The investigation of the properties of gases under the action of the Röntgen rays soon occupied the attention of many workers in the Laboratory. Rutherford, Townsend, McClelland, Langevin, who had come to us from Paris, all were hard at work. C. T. R. Wilson began those researches on condensation which have had so great an influence on the progress of this branch of Physics. A large number of important researches were made which it is not too much to say played a great part in establishing the theory of the conduction of electricity through gases which is now almost universally accepted. It was found that the gas became a conductor of electricity when exposed to the rays, and retained its conductivity after the exposure had ceased for a time sufficiently long to allow of examination of the properties of the gas when in this state. It was found that the conductivity could be filtered out of the gas by sending it through a plug of glass wool or allowing it to bubble through water: thus the conductivity was due to something mixed with the glass. That this something

consisted of particles charged with electricity was shown by the fact that the conductivity could be removed from the gas by applying electric forces to it. These forces would, of course, attract or repel electrified particles and thus withdraw them from the gas; it would not, however, affect particles that were not electrified. Thus the conclusion was arrived at that when the Röntgen rays pass through a gas they produce a supply of positive and negative particles, and these by moving through the gas under electric forces endow it with conductivity. If the gas is left to itself the positive and negative particles reunite, and then the gas gradually loses its conductivity.

The properties of these electrified particles were studied in great detail in the Laboratory, the velocity with which they move through a gas when attracted or repelled by electric forces was measured, the laws according to which the positive and negative particles reunite, the rates at which they diffused through different gases were measured, and in a very short time the properties of these electrified particles in gases, gaseous ions they are called, were known with much greater precision than those of the electrolytic ions in liquids which had been studied for a very much longer time.

The properties of these particles seemed to indicate that they were at least as big as the atoms of the gas in which they were produced; in fact, there were indications that they were clusters of a small number of these atoms. Some experiments I made in 1897 led, however, to the discovery of particles of quite a different order of magnitude. The cathode rays which proceed from the cathode when an electric discharge passes through a tube in which the pressure of the gas is exceedingly low were discovered by Hittorf, and many experiments had been made on them by Goldstein, Varley, and Crookes. Two widely different

opinions were held about them: some physicists maintained that they were analogous to electric waves in the ether, others that they were material particles charged with electricity. Roughly speaking, all the German physicists were of the former opinion, all the English of the latter. As one after another of the properties of these rays came to light some seemed in favour of one view, some of the other. Thus when Perrin discovered that a vessel placed in front of the cathode so as to receive the cathode rays received at the same time a charge of negative electricity, and when I modified Perrin's experiment and showed that however the rays were twisted about by a magnet they still communicated a charge of negative electricity to any vessel they entered, the result seemed almost conclusive in favour of the second view. Then, however, came Hertz's discovery that these rays could pass through thin plates of metal and other substances. This seemed at the time, though it would not now, a strong argument against the rays being material particles, for it was not suspected that these particles could be anything but atoms or molecules, and it was not thought likely that these would be able to pierce solid obstacles. At that time the α particles from radium, which are atoms of helium and which are able to penetrate solid obstacles with greater ease than many kinds of cathode rays, were not known. I had for a long time been firmly convinced that these rays were charged particles, but it was some time before I had any suspicion that they were anything but charged atoms. My first doubts as to this being the case arose when I measured the extent to which the rays were deflected by a magnet; the deflections were far greater than I could account for on any hypothesis which seemed reasonable if the particles had a mass approaching even that of a hydrogen atom, the smallest mass then known. I therefore began to make a series of measurements of the

ratio of the mass of the particles to their charge and also of their velocity, using several different methods. These all led to the result that the ratio of mass to charge for the cathode particles was less than the thousandth part of the same ratio for the atom of hydrogen and its electrolytic charge. Unless, then, the charge on the cathode ray particle were a thousandfold the electrolytic charge the mass of the particles in the cathode rays must be less than the mass of the atom of hydrogen, the smallest mass hitherto recognised by science. The next thing to be done, therefore, was to devise methods by which the charge could be measured. This was done, and it was found to be the same as the electrolytic charge. I could see no escape from the conclusion that in the cathode rays we had particles far more minute than any hitherto recognised. I think the first announcement of this result was made at a Friday evening meeting of the Royal Institution on April 30, 1897. Subsequent investigations have shown that these minute negatively charged particles, corpuscles I call them, are very widely diffused and form a part of every kind of matter and play an important part in many physical phenomena.

To return, however, to the more social matters connected with the Laboratory, by 1898 the number of research students had increased so much that they determined to celebrate the event by a dinner, and the first of what has proved to be an uninterrupted series of such gatherings was held in December, at a restaurant in Sidney Street. I remember that during the songs after dinner, the Proctors came to inquire what the proceedings were about; they did not, however, penetrate to the room where we were dining, being, I suppose, impressed and, I have no doubt, mystified by the assurance of the landlord that it was a scientific gathering of research students. It was at this dinner that Langevin sang the 'Marseillaise'

with such enthusiasm that one of the waiters, a Frenchman, was moved to tears and wished to fall on his neck and embrace him. Now that the number of old research students is so large, these dinners, like the 'Old Boys'' dinners at a college, afford opportunities of meeting old friends, and help to establish a connexion between the past and present workers in the Laboratory.

In recent years a notable feature of these gatherings has been the songs specially composed for the occasion by members of the Laboratory; they deal with topical matters, personal or scientific, and are set to some well-known air. One of the most successful of these songs, written by Mr. Robb, is printed below. For the benefit of non-mathematical readers it should be explained that the chorus represents the way in which 'Maxwell's equations,' the foundation of modern electromagnetic theory, would be pronounced, which the marvellous ingenuity of the author has succeeded in adapting to the needs of the rhythm.

THE REVOLUTION OF THE CORPUSCLE

Air: "The Interfering Parrot." (*Geisha.*)

A corpuscle once did oscillate so quickly to and fro,
He always raised disturbances wherever he did go.
He struggled hard for freedom against a powerful foe—
An atom—who would not let him go.
The aether trembled at his agitations
In a manner so familiar that I only need to say,
In accordance with Clerk Maxwell's six equations
It tickled people's optics far away.
        You can feel the way it's done,
        You may trace them as they run—
$d\gamma$ by dy less $d\beta$ by dz is equal $K.dX/dt$
. . . .
. . . .
While the curl of $(X,Y,Z,)$ is the minus d/dt of the vector
    $(a,b,c,)$.

Some professional agitators only holler till they're hoarse,
But this plucky little corpuscle pursued another course,
And finally resorted to electromotive force,
Resorted to electromotive force.
The medium quaked in dread anticipation,
It feared that its equations might be somewhat too abstruse,
And not admit of finite integration
In case the little corpuscle got loose.
For there was a lot of gas
Through which he had to pass,
And in case he was too rash,
There was sure to be a smash,
Resulting in a flash.
Then $d\gamma$ by dy less $d\beta$ by dz would equal $K.dX/dt$

. . . .

. . . .

While the curl of $(X,Y,Z,)$ would be minus d/dt of the vector $(a,b,c,)$.

The corpuscle radiated until he had conceived
A plan by which his freedom might be easily achieved,
I'll not go into details for I might not be believed,
Indeed I'm sure I should not be believed.
However, there was one decisive action,
The atom and the corpuscle each made a single charge,
But the atom could not hold him in subjection
Though something like a thousand times as large.
The corpuscle won the day
And in freedom went away
And became a cathode ray.
But his life was rather gay,
And he went at such a rate,
That he ran against a plate;
When the aether saw his fate
Its pulse did palpitate.
And $d\gamma$ by dy less $d\beta$ by dz was equal $K.dX/dt$.

. . . .

. . . .

While the curl of $(X,Y,Z,)$ was the minus d/dt of the vector $(a,b,c,)$.

The merits of the advanced students were soon recognised by the colleges. Trinity College elected Rutherford to the Coutts-Trotter Scholarship in 1898, and Townsend to a Fellowship in 1901. Other advanced students who have been elected to Fellowships are H. A. Wilson (Trinity College), A. Wood (Emmanuel College), and F. Horton (St. John's College), while an advanced student, Mr. Craig Henderson, of Trinity College, has been President of the Union. Emmanuel College and Caius College, which have been foremost in encouraging research students and research, offer now regularly scholarships for advanced students. The encouragement given by the colleges in the University to Physics in recent years is, I think, strikingly shown by the following list of students working in the Laboratory, who have been elected to Fellowships during my tenure of the professorship: Anderson (Sidney), Bevan (Trinity), Brand (Pembroke), Callendar (Trinity), Campbell (Trinity), Capstick (Trinity), Chree (King's), Crowther (John's), Fitzpatrick (Christ's), Horton (John's) (Research Student), Moore (Trinity), Peace (Emmanuel), Richardson (Trinity), Spens (Corpus), Strutt (Trinity), Townsend (Trinity) (Research Student), Whetham (Trinity), C. T. R. Wilson (Sidney), H. A. Wilson (Trinity) (Research Student), Wood (Emmanuel) (Research Student). Many of these—for example, Bevan, Callendar, Campbell, Capstick, Crowther, Fitzpatrick, Horton, Spens, Whetham, C. T. R. Wilson, Wood—have taken part in the work of the Laboratory as demonstrators or lecturers.

The number of students is so great that a large teaching staff is required, especially as we have always endeavoured in the practical classes to have one demonstrator to at most twelve students; to do this we generally require ten demonstrators. Again nearly all the lectures on physics in the University are given in the Laboratory.

The two University Lecturers on Physics, G. F. C. Searle and C. T. R. Wilson, each give two courses a year; Fitzpatrick gives a course each term for the first M.B. examination, and Horton a revision course. W. C. D. Whetham, tutor and lecturer of Trinity College, and A. Wood each give every term courses of lectures for Candidates for the Natural Sciences Tripos.

Twenty years ago lectures on Experimental Physics especially intended for students in mathematics were given in the Laboratory by Wilberforce. Their object was to demonstrate to these students the fundamental experiments in Electricity, Light and Heat, so that they should get a more vivid impression of fundamental physical principles than that given by reading accounts of the experiments in a text-book. At that date the curriculum for the Mathematical Tripos was so crowded that students found it difficult to attend any additional lectures, and after a time the class was abandoned. The recent changes in the Mathematical Tripos have altered the conditions, and in the Lent Term 1910 Searle and Bedford organised a series of lectures for mathematical students, which were attended by large audiences and which were very successful; it is hoped that this course will form a permanent part of the instruction given at the Laboratory. Searle has also organised a class in the Long Vacation for science masters who wish to keep abreast with the developments in the teaching of practical physics, and also for mathematical masters who may wish to illustrate their teaching of mechanics by experimental illustrations.

Since I have been Professor there have been two extensions of the Laboratory. The first extension, which was largely paid for by the accumulation of fees from the Laboratory, is the southern wing opened in 1896. This gave us a large room for the elementary classes in

practical physics, a new lecture-room, cellars for experiments requiring constant temperature, and a professors' private room. The second extension, opened in 1908, we owe to the munificence of Lord Rayleigh. This extension, which is to the north of the old Laboratory, provides ample room for those working at research. Before it was built the overcrowding had become very serious. We have generally more than thirty people working at research, and before the new wing was opened the overcrowding was so great that nobody had room to turn, and when at last we got relief I think it would have been impossible to squeeze even one more student into the old building. In addition to rooms for research and a chemical laboratory, this wing contains a large lecture-room and preparation room, a large reading-room, and a room for the use of the demonstrators.

Of the many gifts received by the Laboratory none have been more useful than the apparatus for producing liquid air, presented in 1904 by the President of Queens', who has worked and taught in the Laboratory for more than twenty years. Many of the experiments which have been made in the Laboratory of recent years would have been impossible without its aid.

Another gift, a portrait of myself, painted by Mr. Alfred Hacker, given to the Laboratory in 1902 by past and present students, hangs on the main staircase, and every time I pass it recalls the kindness and good will which it has been my good fortune to receive from everyone connected with the Laboratory. I owe to the Laboratory not merely the opportunities it has given me in indulging my scientific tastes in a way that would have been possible in no other place; it has besides made me rich in friends and in memories of kind acts, good-fellowship and good will which I have received uninterruptedly for twenty-five years.

CHAPTER V

(1885–1894)

J. J. THOMSON was elected in his twenty-seventh year to the Cavendish Professorship of Experimental Physics on December 22, 1884. Lord Rayleigh, his predecessor in the chair, had been elected by the vote of the Electoral Roll. Thomson was chosen by a Board of Electors, constituted, under the new Statute B, 1882, as follows: The Vice-Chancellor (Dr. Ferrers), Professor Clifton, Professor G. H. Darwin, Sir William Grove, Professor Liveing, Professor W. D. Niven, Professor Stokes, Professor James Stuart, Sir William Thomson (Lord Kelvin). The present chapter relates mainly to the first ten years of his professorship. The interval ends just at the time of the discovery of the Becquerel and Röntgen rays; it begins about ten years after the completion of the original building of the Cavendish Laboratory in June 1874.

As my first recollections of the Laboratory date from October 1876, a few personal reminiscences, slender though they be, of those early days may perhaps be forgiven as a preface to this chapter. The only justification for recording them here is that they come from one who can, from his own experience, first as an undergraduate and then as a member of the teaching staff, speak of some of the successive phases of the Laboratory from its earliest days.

Now that the organisation of the teaching and the

From a Photo by W. H. Hayles]

CAVENDISH LABORATORY FROM INNER COURT

opportunities for research are so fully developed, it is not without interest to look back to a time when an undergraduate could find himself dissuaded by his tutor, as I was, from hoping to get benefit in his studies from working in the Laboratory. It seems rather to require explanation how an undergraduate should have had the daring in 1876 to think of 'applying for a place in the Cavendish,' if I may use the expression which was then in vogue with regard to chemical laboratories. To those who are acquainted with the development of the teaching of science in the public schools, the explanation is given when it is stated that the undergraduate in question had just come from Rugby. Amongst the public schools Rugby was one of the pioneers in linking the teaching of science with the learning of it. The boys were given a new kind of opportunity; they were not restricted to sitting in class-rooms and attempting to absorb a small percentage of what was offered in text-books and in lectures not illustrated by experiment; they saw experiments successfully performed by teachers like Neville Hutchinson and James Wilson, and they had access to laboratories wherein they could search for knowledge for themselves.

> It has been the means of educating many boys and has been a visible gain to the great majority. . . . If Dr. Temple had done nothing else, his name would deserve honour at our hands for having brought about this change.—*Nature*, Nov. 1869.

The Science Schools at Rugby were built in 1869, and the new departure was soon established in success. Though (to my lasting contentment, be it said) it was only after my fourth term in the Sixth Form that permission was given me to drop some of the classical work in order to attend Neville Hutchinson's lectures and laboratory-classes, two terms' work in 1876 under his stimulating influence was enough to lead to high expectations of the opportunities that might

be looked for at Cambridge. I can still recall my dismay when in my first week at Trinity in October 1876 Coutts Trotter told me that the Cavendish Laboratory was intended for the research work of graduates, and that there was as yet no accommodation for young students. I wrote in my disappointment to report this to one who was interested in hearing about the opportunities afforded to young students in that line of study at Cambridge: 'For my part I don't see how they can ever hope to get old students, unless they provide accommodation for young ones.' This was a piece of youthful wisdom that was for long used to raise a laugh against the indignant originator of it, who had not then in the least realised how much the Cavendish Laboratory owed to the thought and devotion of Coutts Trotter. When I applied to Garnett he encouraged me to come to the Laboratory, and took much trouble in finding simple apparatus for me to work with. Maxwell came frequently into the Laboratory, but I confess that in my awe of him I used to hide myself motionless behind one of the brick pillars of what is now the elementary class-room, in fear lest he should find me puzzling over things that were made clear in elementary text-books. One day, however—possibly as much to his own discomfort as my own—he took me unawares, and found me struggling with caustics and focal lines. I was working with the concave mirror in the mahogany frame well known to all who have attended the elementary classes. He seemed to be perfectly acquainted with its vagaries, for when I spoke of them he said: 'Yes, I have a shaving glass at home that performs much better. I'll bring it for you.' Ought I to add that he forgot to bring it? At any rate in a moment he had by his simple understanding of the situation turned my awe of his power into enthusiasm for his quiet human helpfulness.

In the search after practical acquaintance with physical methods I was allowed to use a fine balance in the basement-room, where Poynting was making his preliminary experiments with reference to redetermining the mean density of the earth. It was of importance not to disturb the operations: and so before going into the balance-room the procedure was to see if the operator was 'stalking his balance.' There is still, I believe, a hole in the south wall of the balance-room; and it was through this hole that the excessively delicate balance was observed with the help of a telescope by the experimenter, who had to protect his instrument from the attraction and temperature-effects of his own body. Anyone who has realised the heart-breaking disagreement of residuals in work which is pushed close to the limits of the unattainable will understand the sense of security which would be gained by such an arrangement. But I could seldom see these mysterious operations going on without thinking of the story of the Irish Bishop, who found a little boy sitting alone with his back to a wall, afflicted by a head-cold and calling out at intervals pairs of numbers: 'Six five'—sniff—'one four'—sniff, sniff—and so on. 'What are you doing, my little man?' asked the Bishop. 'Sure, they're playin' backgammon over the wall,'—(sniff)—'and I'm the dice, yer honour.' I must leave it to the reader to decide on which side of the wall the dice were in Poynting's game.

The room in which his apparatus was set up was then known as the balance-room. In those days the original plan of the Laboratory was still apparent. Thus the room at the east end of the ground floor was arranged for magnetic investigations. The original purposes of the room have long since been disregarded; it is probably as full of iron and electro-magnets as any other room in the building. And in fact many are the times when strange variations in the

galvanometers in the room above have been traced to magnetic storms issuing from the very room that was built to exclude them. One of the optical rooms on the top floor was used at one time for housing a new form of submarine boat.

In the Lent Term of 1878 I attended a course of lectures which Maxwell gave on Elementary Electricity and Magnetism; the notes which I took show that they followed very closely the MS. for the 'Elementary Treatise on Electricity' which Garnett edited in 1881. The lectures were very attractive, and I still remember being immensely struck with the contrast between the careful precision with which Maxwell chose his words in defining terms, and the somewhat rambling remarks that he made in explaining the use of them. He spoke quite informally as if conversing with a friend, and sometimes even as if speaking to himself. Every now and then a humorous remark would fall from him to the obvious bewilderment of some in the small audience, but much to the unrestrained amusement of Garnett, who nearly always attended the lectures, sitting in a chair on Maxwell's side of the lecture table.

In illustration of Maxwell's precision in defining terms, I recall to this day the sudden illuminating effect (in the mind of a raw student) of his few words in explanation of the definition of magnetic field. He was very fond of using words like '*the quality or peculiarity in virtue of which*'; thus he would define electromotive force as the quality of a battery or generator, in virtue of which it tends to do work by the transfer of electricity from one point to another. So in defining a magnetic field as a region possessing or possessed of a peculiarity he cautioned us against thinking that a field of the strength of so many C.G.S. units meant so many dynes; the field is 'there,' whether or not an isolated unit magnetic pole be set in it to experience

the force of so many dynes; the dynes came in by reason of our arbitrary choice of a special aspect of the peculiarity of the field; the choice had to be justified by consistency in other relations, such as in the phenomena of the rotation of the plane of polarisation of light. His exact words I cannot of course recall; but, as I have said, the impression of an illuminating flash was produced, and the raw student was made in an instant to wonder whether the entry of dynes into the measure of gravity did not leave the door open for glimpses of similar outstanding differences in peculiarities in the field of gravitation.

Nowhere in his writings is this precision of thought and clearness of statement more remarkably exemplified than in Appendix C, 'On the elementary relations between electrical measurements,' published under the names of Clerk-Maxwell and Fleeming Jenkin in the Second Report (1863) of the Committee of the British Association on Electrical Standards, and reprinted under the editorship of Fleeming Jenkin in 1873. It remains a model of precise statement, which all students would do well to study. It would be difficult to find so marked an example of the kind of spirit, which Maxwell's influence left as a heritage specially to be cherished in the Cavendish Laboratory.

The article to which I refer begins with words which recall Maxwell's characteristic expression and thought:

1. *Object of the Treatise.*—The progress and extension of the electric telegraph has made a practical knowledge of electric and magnetic phenomena necessary to a large number of persons who are more or less occupied in the construction and working of the lines, and interesting to many others who are unwilling to be ignorant of the use of the network of wires which surrounds them. . . . Between the student's mere knowledge of the history of discovery and the workman's practical familiarity with particular operations which can only be communicated to others by direct imitation, we are in want of a set of rules or rather

principles by which the laws remembered in this abstract form can be applied to estimate the forces required to effect any given practical result.

We may be called on to construct electrical apparatus for a particular purpose . . . If we are unable to make any estimate of what is required before constructing the apparatus, we may have to encounter numerous failures, which might have been avoided if we had known how to make a proper use of existing data.

All exact knowledge is founded on the comparison of one quantity with another. In many experimental researches conducted by single individuals, the absolute values of those quantities are of no importance; but whenever many persons are to act together it is necessary that they should have a common understanding of the measures to be employed. The object of the present treatise is to assist in attaining this common understanding as to electrical measurements.

Then follows the treatise in about thirty-five octavo pages, as brilliant a piece of elementary teaching as was ever published. Could one have a more telling expression of faith from a master of science, showing in a really vital way his view of the importance of getting practical engineers to base their procedure on a real understanding of elementary scientific principles? But I must not allow myself to be drawn aside into a digression on the importance of a strictly scientific basis in technological education.

Research work was being carried on by Chrystal, Schuster, Gordon, Poynting, Glazebrook, Fleming, and others, but apart from an occasional envious visit to one of these, it was lonely work for an undergraduate, who tried to gain a knowledge of physics by practical experiments. The wisdom of Coutts Trotter's unpalatable advice to wait a year or two for the organisation of regular classes became sadly clear, in spite of the interest of trying to play teacher to oneself, and the interest was deep enough to give rise to the wish that all triposes were at the bottom of the sea and that the work

then to be done might have been to help in devising a course of experiments for the illustration of methods of physical measurement. Looking back I can only hope that the mêre presence of a forlorn undergraduate in the Laboratory in 1876 and 1877 may have had some influence in hastening the organisation. Though formal notice was given in every term in the *Reporter* that the Laboratory was open to students, it was not until the Michaelmas Term of 1879 that the first organised course of practical experiments in physics was advertised and given. I did not attend this course, but I must have been working in the Laboratory during that term; for I recall the deep impression produced by the hopeless news of Maxwell's illness, and the wide-spread mourning for his death, which occurred on November 5.

In the following year (1880), the first of Lord Rayleigh's régime, the courses were initiated by Glazebrook and Shaw and soon were put on to the lines that have persisted to this day. The immediate increase in the number of students in the Laboratory was strong proof of the existence of a demand for the sort of courses then organised. By the beginning of 1881 there were thirty-four students attending the classes, advanced and elementary. The point of view changes quickly, and I recall how those of us who had recently ceased to be undergraduates were ready to temper their satisfaction in the increasing number of students by the reflexion that at times they were a source of considerable inconvenience; for it meant that many instruments, which would have been very useful for our purposes, were permanently allocated to the use of the classes. If any of us was bold enough—and there were not a few such—to purloin an instrument for 'private' and temporary use, it was always at a critical moment in the measurements that a messenger would arrive bearing Mr. So-and-So's

'compliments and would,' &c. Another day the messenger would omit the compliments; and if this procedure failed to divert the private research to a time that did not clash with the requirements of the classes, Mr. So-and-So would arrive in person and reduce the purloiner to a proper state of contrition.

When I returned to Cambridge in 1885, after an absence of four years, the signs of activity were very striking. The number of students attending the demonstrations had increased to ninety and there were about ten students engaged in original research. Glazebrook and Shaw had two assistant demonstrators to help in conducting the classes. The organisation was strikingly complete, both in the elementary and in the advanced classes.

This then brings me to the beginning of the period, which is the proper theme of this chapter. I propose first to deal with a brief outline of the continuation of the teaching which had been completely organised in Lord Rayleigh's régime, and then to attempt to recall something of the varied research work done in the Laboratory between the years 1885 and 1895.

To one who returned to Cambridge in 1885 after an absence of four years, the organised activity in the Laboratory was very striking. Symptoms of the lines along which the development had taken place were visible in the shape of the note-books prepared to assist students in carrying out the different experiments, which had been chosen by Glazebrook and Shaw as most suited to give a thorough working knowledge of the elements of experimental physics. Some of these note-books were in MS. in the handwriting of Glazebrook and of Shaw; others contained proof sheets which had replaced the original MS. The proof sheets were those of Glazebrook and

Shaw's book on 'Practical Physics,' which was published in the beginning of 1885.

The method adopted by Glazebrook and Shaw of conducting the elementary classes gave a student a wide survey over many branches of elementary physics. It involved, however, that there was in general no immediate relation between his work in the laboratory and the lectures that he might be attending. Any attempt to co-ordinate the laboratory work with the subject of lectures necessitates the multiplication of sets of apparatus required for the class. But if this difficulty—which is one of ways and means—can be overcome, there are great advantages to the students in many ways. Firstly, the lecture work is at once a preparation for the work in the laboratory; secondly, any difficulty experienced by a student in the subject of the lecture can be immediately dealt with by the demonstrator in charge of the laboratory classes; thirdly, the practical work in the laboratory can be organised in such a way that any student can derive benefit, not only from his own experiment, but also from comparison or contrast with the results obtained by his fellow-students; and, most important of all, the energy and interest of the students are undoubtedly quickened by the sense of co-operation in working actively towards what is really a verification of a broad scientific principle.

I hesitate to dwell here on matters that strictly speaking form the subject of a later chapter in this volume. But as I believe the first attempts to carry on a class on the lines just indicated were made during the time that I held office as demonstrator, it has seemed permissible to refer to the matter here. The attempts arose out of a suggestion made by Shaw shortly after he had been appointed University Lecturer in Physics in 1887, and we made the first trials when he was lecturing on electricity and magnetism. A single instance will serve to indicate the method tried. We

may take the case of a lecture given by Shaw on intensity of magnetisation of iron and the meaning of k the coefficient of magnetisation. The laboratory work for that day was arranged so that there were six or eight sets of apparatus, consisting of battery, galvanometer, a magnetising coil, a bar of soft iron and a magnetometer, but the battery power and the number of turns of wire in the magnetising coils were different in the various sets. Twelve or sixteen students worked in pairs; each pair had enough to do (in the $1\frac{1}{2}$ or 2 hours devoted to experimental work) in setting up their apparatus and determining the value of k for one value of the magnetic field. At the end of the morning's work, the various pairs of students presented their results, which were at once checked and, if necessary, corrected, and a graph was made co-ordinating the different values of k obtained by the different experimenters with the strength of the magnetic field used. Thus each pair of students got practical acquaintance with the method of determining k for a special value of the field, and the whole class contributed to the verification of the well-known results relating to the gradual saturation of iron in increasing strengths of magnetic field. Similarly when Shaw's lecture dealt with Faraday's laws of electrolysis, then in the practical experiments some students worked with a gas voltameter, others with copper voltameters, others with silver voltameters. Each pair of students obtained their own special results and derived benefit from comparing them with those got by other students.

There can be little doubt that it adds much to the interest of the students to work in a class conducted on these lines. There seems to be a certain exhilaration introduced by the co-operation and competition. And, moreover, it affords the teacher opportunities of showing the students that the apparatus provided has to be chosen with some forethought. For he may assign to one pair of

students apparatus which he knows is not well chosen, and the untrustworthiness of results can be explained to the students far more effectively after actual experience of failure in one set of apparatus and success in another. Thus in the case of the copper voltameters, in many cases a student is in danger of thinking that the electrochemical equivalent of copper can be satisfactorily determined with vessels of any weight and with any current density. In a class such as has been described, the teacher (if he is careful in exercising judgment in assigning special apparatus to special students) may show (1) how if the cathode is too big and the current too small the deposited copper is too small in quantity to give a satisfactory determination and (2) how if the cathode is too small and the current too big the copper deposited is spongy or powdery and so cannot be washed and dried for the purposes of weighing. Such experiments would occupy a single student for too long a time; but when a student, after working with inappropriate apparatus, can see the results obtained by fellow-students who have had properly designed apparatus assigned to them, he is in a position to deduce his numerical results from his fellow students' weighings &c. Should it happen that the class does not contain men who would enter properly into the spirit of the chase, the teacher can always himself take charge of the faulty experiments to show the need for forethought in designing the necessary apparatus.

It may be doubted perhaps whether the method of combined work is not more suited to advanced classes than elementary. But in spite of the organisation of the courses of advanced work which Glazebrook and Shaw developed, and in spite of the greater openings for combined work, it seemed that the differentiation in the aims of men in the advanced work was already too great, and the claims of other subjects than Physics on their time made it difficult to

devise a satisfactory plan of simultaneous co-operation in experiment.

It seemed now and again that a great desideratum was that the work that Glazebrook and Shaw had devoted in organising the advanced courses should be rendered more generally available both to students and to teachers by the publication of a text-book in advanced work. But the essence of their method was reference to original memoirs, and there can be but little doubt that this method leads best to the easy development, not only of the instinct of research, but also of individuality in the investigator.

Certainly there was room for the summarising of the results got by the different students in repeating a given experiment; but though to my knowledge this was recognised again and again, it always happened that the demonstrator who seemed the right man to undertake the work either got appointment elsewhere or else had other duties devolving upon him that seemed to have prior claims upon his time and energy. In one or two instances the summarising was done. I may take the case of the standards of electromotive force. Each student that worked in the advanced class in electricity and magnetism made a Clark cell, tested it, and recorded its performance at the time of its being made. Some latitude of variation in following the prescription was allowed provided it was recorded. Such cells were preserved as far as possible together with the records; and they were, to the number of 30, systematically compared by Glazebrook and Skinner in their work on 'The Clark Cell as a standard of E.M.F.' (*Phil. Trans.* 1892).

When once the Tripos Examination was passed, post-graduate study began for those who chose to embark upon research, and such men, whether they realised it or not, were in reality joining a band of workers who were together engaged in combined advance under the leadership of Thomson

or Glazebrook or Shaw. The strength of the situation was that the advance was made without the slightest sense of fetterdom, and that the individual was left free to search for his own inspiration or to get it from one of his leaders, and then to work out the solution of his problem. Help was generally forthcoming when difficulties began to seem unsurmountable, and the greater the sense of comradeship the quicker the advance. It is pleasant to read in a paper recently communicated to a scientific periodical the final paragraph in which the writer (an American) acknowledges his indebtedness to those who have helped him and concludes by recording 'his tribute to the admirable spirit of comradeship in research which is found in Cambridge and especially in the Cavendish Laboratory.'

Those who worked in the Laboratory in the years 1885–87 will recall with pleasure the ever-ready help which they received from D. S. Sinclair. As attendant in general charge of the Laboratory he would search out apparatus or devise and make with a surprising rapidity (which vied almost with that of Gordon, Lord Rayleigh's assistant) any accessories needed for the varied experimental work in train. He was a very cunning glass-blower. One of my correspondents recalls to my mind the picture of Sinclair standing on one leg, propped against a high stool, working the bellows of a blow-pipe with the other foot, twirling a complicated piece of glass apparatus in his hands, holding in his mouth a rubber tube, with which he could prevent undue shrinkage in the glass vessel, one eye fixed intently on his work, and the other twinkling with some temporarily suppressed waggishness. I remember how, when he unpacked a very light aluminium anchor ring which had been made for some of the Professor's electrical experiments, he found that a small piece of solder had got loose in the hollow

ring. He shook the tube and said with northern emphasis of the 'r,' and with waggish solemnity, 'and that's the vector potential.'

After Sinclair came A. D. Bartlett, who was an expert in winding coils and in making and adjusting resistances. He remained from 1887 to 1891, and was followed by W. Pye, who had received his training in Mr. Horace Darwin's workshop at the Cambridge Scientific Instrument Company. His tenure of office as attendant was marked by the excellent organisation of the workshop department of the Laboratory at a time when much apparatus was needed for the increasing classes under Fitzpatrick.

This section of the chapter may be closed by a reference to Thomson's lectures. They were very much appreciated by the students, whether as guidance in reading for Tripos work or for suggestion of research in post-graduate study. His power of suddenly introducing an old friend in a new light, his patience in remembering that many of his hearers had not had a systematic training in mathematics and would always be the better for being led *from first principles* to the point of view that he wished to emphasise, these were qualities that made his lectures supremely valuable to students both in mathematical and experimental physics. The speed and accuracy with which he would work out mathematical points on the blackboard were exceedingly striking. If I may be allowed to pass abruptly from grateful recollection of the lectures to memories of the amusing touches of someone who with the irresponsibility of early post-graduatedom contributed a letter to a series that appeared in the *Cambridge Review* addressed to various teachers and professors, I hope the Professor will forgive me for quoting the following paragraph :—

It is very apparent that you are always physicist first and

mathematician second. For when in the course of some investigation a new function turns up which would keep some of your colleagues at Trinity contented and happy for months, you merely 'with a grave scornfulness' select such of its properties as you require and march straight on to the goal you have in view. And this accounts for what sometimes befalls you in lecture-room. For, though knowing well what is the result you wish to obtain, you have occasionally mislaid the envelope back containing the details of the investigation and are compelled to plunge at short notice into a sea of symbols. Yet when, since even Professors make slips sometimes, it becomes evident that the desired result is not coming and you find it necessary to apply an empirical correction to the work on the blackboard, the cool confidence with which you say 'Let's put in a plus' and the smile of cheery conviction with which you turn to your audience put to shame the incredulity of the most sceptical among them.

I am the less unwilling to recall the unblushing boldness of that letter, for, to many of those who read it in the days when they attended the Professor's lectures, it will appeal in a different way now that they have themselves had the experience of lecturing.

There is a paragraph in the introduction to his book on the 'Applications of Dynamics' &c., which may here be quoted with one word italicised. It runs as follows:—

> The object of the following pages is to endeavour to *see* what results can be deduced by the aid of these purely dynamical principles without using the Second Law of Thermodynamics.

The use of that word 'see' instead of the word 'show' seems to me to illustrate completely the secret of the Professor's power as a teacher; it expresses in a word his sympathy with his hearers, he makes himself one with them and proposes that they should join him in looking.

In the remaining pages of this chapter, some attempt has to be made to give an idea of the research work carried on in the Laboratory in the period 1885–1894.

The statistical dry bones of the number and nature of the communications to the publications of various societies and to scientific periodicals may be summarised as follows:—

Total number of communications—

220 from about 36 contributors.

This number includes short notes, abstracts, memoirs, reports and summaries, but does not include many papers of a mathematical and theoretical nature that did not lead immediately to experimental work.

The distribution amongst the various branches of physics may be indicated by the following table, relating to the more important papers:—

General Physics and Properties of Matter	13 per cent.
Heat	9 ,,
Optics	9 ,,
Electricity and Magnetism	20 ,,
Conductivity of gases	20 ,,
Non-experimental	10 ,,
Meteorology	2 ,,
Reports and Summaries	7 ,,
Miscellaneous	10 ,,

It is seldom an easy matter to project one's mind back to a special epoch in such a way as to realise the state of affairs at the time, and it is especially difficult to do so, when the epoch chosen is one in the midst of a widespread movement of great activity. If an historian of recent physical science had to choose the epoch from which he must date his history, I can hardly believe that he would begin with the year 1885. Yet my allotted task is to give some account of the work in the Cavendish Laboratory dating from that year.

The danger in such a task is that there is a strong

temptation to make the history self-contained in the sense that every step shall seem to be the consequence of previous steps within the community. When one begins to look around and to widen the scope of the survey of contemporaneous activity outside and to try to give due weight to its influence, there arises the same instinctive tendency towards economy in limiting the system to be considered as there is in physical science itself, or at any rate as there was in physical science before J. J. Thomson showed us how to bring complicated phenomena within the scope of the method of Lagrange's Equations.

The only way that seems likely to meet the present case is to appeal to the memory of my readers. As when, in recalling to a friend the beauties of a mountain walk taken many years before, the mention of one memory awakes another till wholly forgotten scenes are vividly brought back to mind, so I must proceed here. A reference to a patch of gentians will recall a glimpse of distant mountains through dissolving mists. The rough stony path will serve to bring back the recollection of flowery pastures and springy turf. 'That chalet where we got the scented milk and had to crawl up to the col again' will remind us of a missed track as well as the view into the promised land over the chestnut woods.

In some such way as this, I would endeavour to conjure up something of the outlook that was before the eyes of those who were in the rank and file at the beginning of Thomson's professorship. The earlier chapters in this volume bring before us the nature of the activity within the community. Here I would jot down a few of the more recent advances achieved outside; then, after a brief reference to what was soon to be published, I would dwell for a moment on the array of 'reports on recent progress' which emanated from members of the staff of the Cavendish

Laboratory in the earlier years of the period to which my summary relates.

Among recent advances, the following may serve to recall the position of science:—

1873. Kohlrausch had published his work on the velocities of the ions in electrolytes, in extension of Hittorff's earlier work (1855). The Clausius-Williamson hypothesis (dating from 1858) was in full possession of the field.

1874. Kerr had discovered the effect of magnetisation on the plane of polarisation of light reflected from iron.

1876. Rowland had demonstrated the magnetic effects of electrical convection.

1877. Pictet, at Geneva, and Cailletet, at Paris, had succeeded almost simultaneously in liquefying oxygen.

1875-9. Crookes had published his researches on the radiometer, the trajectories of molecules, and the physics of high vacua.

1880. Wet plate photography was being displaced by the invention of the dry plate processes.

1882. Faure's accumulators came upon the scene. It was in 1884 that Sir W. Thomson presented Peterhouse with the first electric-light installation in Cambridge.

1881. Helmholtz delivered his Faraday lecture.

1881. The Hall effect was discovered.

1882. Elster and Geitel were embarking on their investigations of the electrification of gases by glowing metals.

1883. Blake had shown that the vapour of mercury arising from boiling mercury was not electrified, however strongly electrified the mercury might be.

1880. Goldstein's researches on 'that remarkable motion which radiates from the kathode in rarefied gases' had appeared in translation in the *Philosophical Magazine*.

These references may, I hope, serve to recall to my readers—*chacun à son goût*—some of the features in the landscape along the track of advance. Ahead and still veiled behind the clouds were many a fine peak. If I name

one or two, others as notable or even more striking will probably come into the mind of the reader:—

Poynting was working out his theorem as to the direction of the flow of energy in an electrodynamic system, and published it in 1885
In the same year Ewing's researches on the magnetisation of iron began to appear
Quartz fibres were not spun from Boys's bow till . . 1887
Van't Hoff was applying the principles of thermodynamics to explain Pfeffer's (1877) phenomena of osmotic pressure, but his results were not to appear till . . 1887
Arrhenius had written his inaugural dissertation in 1883 and was working up the accumulation of evidence in favour of the theory of dissociation of salts in solution. His leading memoir was published in . . . 1887
Hopkinson was studying the magnetisation of iron at high temperatures, and published his results in . . . 1889
Ewing did not publish his molecular theory of magnetism till 1890
Rowland's map of the solar spectrum was not issued till . 1887
Kayser and Runge only succeeded in establishing the existence of order in spectra in 1888
Hertz's work on electrical waves was published in . . 1887 and thereby the faith in Clerk Maxwell was suddenly converted from a faith that was nurtured by hope into a faith that was based on conviction.
Drude's work on the application of the electromagnetic theory of light to explain the scattering of light from small particles and the reflexion of light did not appear till 1890-2
Schuster's second Bakerian Lecture on electric discharge in gases was delivered in 1890

Within the community itself, there were signs of directed and directing activity of an unusual kind, in 1885. There were of course hundreds of plots waiting to be occupied in the fertile ground that Maxwell's genius had opened out, in molecular physics, in electrical theory, and in electrical practice. There were hundreds of paths to be followed,

suggested by the concentrated work of Lord Rayleigh in probing to their depths the experimental possibilities of deciding between conflicting experiment and unreconciled theories. It would be difficult to recognise in suitable words the debt that the Cavendish Laboratory and Physical Science owe to the devoted labours of Lord Rayleigh. The stimulating influence of his tenure of office as Cavendish Professor and the continued interest he afterwards showed in the welfare and progress of the work of the Laboratory made it difficult not to include his papers among those of the period 1885–1894. Had they been included, the number would have been increased by 141. During his professorship he contributed 75 memoirs and notes to scientific periodicals. Probably no report could be imagined as more certain to further the advance of science than one by Lord Rayleigh himself on his own work, a report that should deal with the history of the various lines of thought and should put the various memoirs (there are 384 in the four volumes of his Collected Papers up to 1901) into their proper relation to one another. Such an account would be of immense value, not only to the mathematical physicist but also to that large body of workers, who may be compared with those who in mountainous regions love to search out the green passes rather than attempt high peaks. I make bold to write such a suggestion here, for I feel that I plead for numberless students for whom the green passes are generally a necessary preliminary to the ascent of high peaks.

Into the midst of the wealth of choice of subjects for research afforded by the work of Maxwell and Rayleigh, there came

R. T. Glazebrook's Report on Optical Theories (B. A. Report 1885),
J. J. Thomson's Report on Electrical Theories (B. A. Report 1885),

summarising the position of affairs in these two subjects in a way that could not fail to guide experimental work towards criteria between surviving choices.

In Pattison Muir's new edition of 'Watts' Dictionary of Chemistry,' which appeared about that time, several of the articles on physical aspects of the subject were contributed by Thomson.

Soon also there was to appear the report on Electrolysis that W. N. Shaw was working at with the co-operation of T. C. Fitzpatrick and others (B. A. Report 1890). H. L. Callendar was at work in his search for a method of standard thermometry that should vie with the electrical standards already being issued from the Cavendish Laboratory.

For such investigators as were tempted to study the properties of matter, there were J. J. Thomson's lectures and memoirs on 'Applications of Dynamics to Physics and Chemistry.' These were published first in the *Philosophical Transactions* for 1886 and 1887 and were later recast in book-form (Macmillan, 1888).

In his preface to the book, Thomson says:—

> There are two modes of establishing the connexion between two physical phenomena; the most obvious as well as the most interesting of these is to start with trustworthy theories of the phenomena in question and to trace every step of the connexion between them. This, however, is only possible in an exceedingly limited number of cases, and we are in general compelled to have recourse to the other mode in which, by methods which do not require a detailed knowledge of the mechanism to produce the phenomena, we show that, whatever their explanation may be, they must be related to each other in such a way that the existence of the one involves that of the other.

The principal advances in Physical Science lead us to believe that all physical phenomena can be explained by dynamical principles. The researches of Davy, Rumford,

Mayer, Joule, and others showed that the energy which moving bodies possess by reason of their motion can be converted into heat. Joule's discovery of the constant relation between the quantity of kinetic energy which disappears and the quantity of heat that appears in consequence showed that the principle of the Conservation of Energy holds in the transformation of kinetic energy into heat and *vice versa*. Helmholtz summarised, in his treatise 'Ueber die Erhaltung der Kraft,' 1847, the various transformations in the different branches of physics and showed that many well-known phenomena are connected in such a way that the existence of the one involves the other. Maxwell, in his memoir on 'The Dynamical Theory of the Electromagnetic Field,' 1864, pointed out that by the proper use of terms like electric momentum and electric elasticity it was possible to interpret the phenomena of the induction of electric currents and the polarisation of dielectrics, and he then showed how, by the application of Lagrange's equations to the connected system thus recognised in the electric field, he could deduce the mechanical and electrical actions by means of the fundamental principles of dynamics. J. J. Thomson, using the idea suggested by Maxwell's work, shows in his turn how by proper generalisation of co-ordinates in Lagrange's equations or Hamilton's principle of varying action we possess methods that do not require a knowledge of the intimate mechanism of the systems to which they are applied:—

Thus, to take an illustration, suppose we investigate experimentally the effect of a current of electricity both steady and variable upon the torsion of a longitudinally magnetised iron wire along which the current flows, then we can deduce by dynamics the effects of torsion and variations of torsion in the wire upon a current flowing along it.

In ordinary dynamics of a rigid body the term co-ordinate

is used for a geometrical quantity helping to fix the geometrical configuration of the system. When we apply dynamics to physics, the configurations have to be described, not only geometrically, but also with respect to such things as distributions of electricity and magnetism. We have, in fact, to use co-ordinates which are not geometrical but may be of any type. J. J. Thomson specifies five different kinds of co-ordinates as sufficient for his purposes, thereby defining the following configurations: (1) geometrical, (2) strain, (3) electrical, (4) magnetic (two co-ordinates). He shows how to apply Lagrange's dynamical equations in a generalised way. Thus, if T and V are respectively the kinetic and potential energies of the system and q is a co-ordinate of any type, Lagrange's equations can be written in the form

$$\frac{d}{dt}\frac{d\mathrm{L}}{d\dot{q}} - \frac{d\mathrm{L}}{dq} = \mathrm{Q}$$

where L is written for T—V and is called the Lagrangian Function and Q is the external force acting on the system tending to increase q. Here the kinetic energy T is to be expressed in terms of the velocities of the co-ordinates, but in many cases it is convenient to work with the velocities corresponding to some co-ordinates, but with the momenta corresponding to the others. Thomson shows that in such cases Routh's modified function, L′=T—V—twice the kinetic energy corresponding to the co-ordinates whose velocities are eliminated, will serve to define completely the 'motion' of the system. In using the method we have to write down a general expression for the modified Lagrangian Function; and in deciding whether individual terms exist among the squares and products of velocities or momenta corresponding to the different kinds of co-ordinates, we make our appeal to our knowledge of physical systems. In fact, if the existence of any particular term

would involve physical consequences which we know to be contrary to experience, we conclude that this term does not exist.

The method is one of great comprehensiveness, and Thomson sets it forth in his book with a wealth of illustration and applications to various cases. It must be remembered that the object of the method is not to discover the properties of physical systems in an altogether *à priori* fashion, but rather to predict their behaviour in certain circumstances after having observed it under others.[1]

For confidence in the results deduced by the method we must be sure of the completeness of the material put into the mathematical mill. We must be ready to extend the system, which we consider in any problem, widely enough to be sure that we do not overlook the passage of energy in disregarded directions. The power of the method lies in its affording means of estimating the magnitude of a suspected effect which we may be tempted to look for. For instance, we know how by the aid of an external magnetic force we are able to rotate the plane of polarisation. Thomson shows by the application of his method that, though a circularly polarised beam of light must necessarily produce a magnetic force parallel to the direction in which it travels, the force is too small to be detected when light as strong as sunlight is used.

When Hale discovers signs of the Zeemann effect in the spectra of sunspots, and attributes them to the existence of magnetic vortices, Thomson's method seems to induce a

[1] The irresponsible author of the letter in the *Cambridge Review* to which I have already referred addressed Professor Thomson as follows :—'You have applied Dynamics to Physics and Chemistry ; why not extend the application still further ? This is the task I propose to you—to find the Lagrangian Function of the University. Think of the advantage of being able to obtain by a simple mathematical process the Report of a Syndicate on any matter, for such things, I imagine, would correspond to equations of motion.'

desire to know the solution of the question 'Which comes first, the hen or the egg?' But this is a question which was propounded before 1885 and is recalled by observations made after 1895 and must therefore be dismissed as being irrelevant.

In the almost hopeless task of giving an idea of the 200 papers relating to the period 1885–94, I will divide them according to subjects.

Experimental optics.—In experimental optics, McConnel's work on the form of the wave surface in quartz was a natural sequel to Glazebrook's earlier work on Iceland spar, in which experiment was needed to decide between conflicting theories. The observations of Stokes and Glazebrook showed that in Iceland spar the Huyghenian construction is a very close approximation to the truth. McConnel attacked the question of the form of the wave surface in quartz by measuring the dark rings formed in sodium light by plates of crystal cut in different directions with respect to the axis, thus extending the work of Jamin in 1860. Each ring is due to one wave being retarded in the quartz behind the other by an integral number of wave lengths, so the measurements give the directions through the plate of quartz corresponding to a series of known retardations. The relative retardation is, especially in a crystal of weak double refracting power like quartz, mainly dependent on the distance between the two sheets of the wave surface. Thus McConnel's observations give the separation between the two sheets at various points, and it is in this separation that the peculiarities of quartz are most strongly marked and the various expressions put forward by the theory most widely divergent:—

In quartz the wave surface may be nearly represented by a prolate spheroid surrounded by a sphere passing through the extremities of its axis. The spheroid is slightly flattened at

the extremity of its axis, and the sphere slightly bulged, so that the two no longer touch. The distance between the two at the extremity of the axis we know very accurately from observations on the rotatory power, while the two radii at the equator of the wave surface are known from operations with the spectrometer.

In all the theories on the subject these three constants are assumed: so that theoretical surfaces are made to coincide with the true one in the equatorial section and to give the true distance between the two sheets at the extremity of the axis. At intermediate points their correspondence with the true surface lies open to the test of experiment. McConnel's earlier observations in 1883 were confined to the regions near the axis, as it is only there that the bulging and flattening are at all conspicuous. But the increased facilities of a new arrangement for measuring the rings led him to extend his observations over the whole surface, and thus he determined the gradually increasing distance between the two sheets from the axis to the equator.

McConnel found he was able to get readings of the rings up to the 163rd. With plates cut perpendicular to the axis he got values of the separation of the wave surfaces from $\phi = 4^\circ$ to $\phi = 39^\circ$, ϕ being the angle between the ordinary wave normal and the axis; and with plates cut parallel to the axis he dealt with the separations from $\phi = 53^\circ$ to $\phi = 90^\circ$ (equator).

McConnel compared the results of his observations with the theoretical results based on nine theories, and found that they agreed best with the theory of Sarrau (1868). We may utilise Glazebrook's 'Report on Optical Theories' (1885) to see that in Sarrau's theory the ether is supposed to vary in density in a periodic manner from point to point, being arranged in shells of variable density round each molecule of matter.

McConnel left Cambridge in 1885 to go into business, but shortly afterwards he had, on account of illness, to go to a health resort in Switzerland. He died at Davos in 1890 in his thirtieth year. In Perntner's 'Meteorological Optics' (Part IV. 1910, p. 643) we find references to McConnel's work in Switzerland:—

> To McConnel we owe interesting observations on the effect of the nature of the ground on the degree of polarisation of the sky at 90° from the sun on the vertical through the sun. In particular he has noted the influence of snow at St. Moritz, Thusis, Davos, and found that the degree of polarisation is diminished by the covering of snow.

Again in Perntner's book (Part III. 1906) we find references to McConnel's observations on iridescent clouds. His conclusion was that the colours were due to diffraction by ice needles in clouds, and they appear in flecks and not rings, because the clouds are far from the sun and do not wholly surround it. When such colours appear at 20° and more from the sun, the maxima must be of fairly high order, as for instance 5th or 6th. The smallest ice crystals measured on Ben Nevis were 0·0074 mm. For such the 5th maximum in the ring system would appear at 23° from the centre. But the difficulty then is that the intensity should be only half a hundredth of that at the central spot.

McConnel also made very valuable observations on the plasticity of ice at temperatures below freezing point, and showed that the observed bending of ordinary ice at temperatures where regelation was out of the question was to be attributed to the slipping of crystals over each other, ice being an agglomeration of separate crystals. A single crystal when stretched by tension at right angles to the optic axis shows no such yielding, and is perfectly elastic in the sense that it reverts to its original length when released from tension.

The laws of reflexion and refraction were satisfactorily explained by Huyghens on the principles of the wave theory, at least so far as the directions of the rays are concerned. But there have been throughout the history of physical optics outstanding difficulties when the ratio of the quantity of reflected light to the incident light is considered in terms of the nature of the media on either side of the reflecting surface. Fresnel's expressions, based on insight rather than strict reasoning, have been subjected to searching criticism, both theoretically and experimentally. These expressions for the ratio of the amplitudes are—

for light polarised in the plane of incidence, $\frac{\sin(\theta - \theta')}{\sin(\theta + \theta')}$

and

for light polarised perpendicularly to that plane, $\frac{\tan(\theta - \theta')}{\tan(\theta + \theta')}$

At normal incidence, the expressions coincide with Young's expression $\frac{\mu - 1}{\mu + 1}$.

At other incidences, the sine expression increases regularly from $(\mu - 1)/(\mu + 1)$ to unity; but the tangent expression falls slowly from $(\mu - 1)/(\mu + 1)$, till at incidence such that $(\theta + \theta') = \pi/2$ (or $\sin \theta' = \cos \theta$, or $\tan \theta = \mu$) it vanishes, and as the incidence increases, its value rises to unity at grazing incidence.

If then ordinary light falls on such a surface, at incidence $\tan^{-1}\mu$, the reflected light is completely polarised in the plane of incidence, for the other component does not exist. Brewster's discovery of the wide applicability of the relation $\tan \theta = \mu$ for defining the polarising angle, shows that for the angle $\tan^{-1}\mu$ there is at any rate a minimum in the amount of reflexion of light polarised perpendicularly

to the plane of incidence. As the incidence passes through the polarising angle, the reflected vibration changes sign and increases in magnitude, becoming unity at grazing incidence.

The discovery of minute divergences from what appears to be a first approximation to a general law means the discovery of a criterion by which discrepant theories may be put to the test. Jamin and Quincke in 1860 showed that in most cases the reflected beam is elliptically polarised, even when the light is reflected from the surface of transparent media. Thus it would appear that in the phenomena of reflexion were to be found means of deciding experimentally between the results of different theories of that relation between ether and matter which gives rise to optical differences between transparent media. The difficulty in theory resides in deciding how to treat the interface, whether the transition is abrupt or whether there is a layer in which the transition is gradual or the density varies with the time. The subject is one which Lord Rayleigh has worked at with a splendid persistence. His discovery that the state of freshness and cleanliness of the reflecting surface has a large influence, not only on the amount of light reflected, but also on the nature of the elliptical polarisation, seemed to show that Jamin's results could be attributed to the existence of a thin film of impurity on the reflecting surface. It was in 1886 and 1892 that his experimental work was published, following on theoretical work that was begun in 1871 and 1872. Glazebrook called attention to the effect of moisture in modifying the refraction of plane polarised light through a prism of small angle (1884).

Spurge (1886) showed that the process of polishing the natural face of a crystal of Iceland spar certainly alters the state of the surface, and produces a change both in the

ratio of the axes and in the azimuth of the major axis of the elliptically polarised light. Rayleigh continued his own line of attack, and used liquid surfaces, on the ground that they could be easily contaminated or cleaned. His results seemed to confirm completely the view that the difference between a fresh surface and an old one was to be attributed to a film of impurity. But Lummer's results, published in the present year 1910, show that the subject is still not closed, for he finds that the phase is altered by pressure.

Electro-optics.—On the borderland between Optics and Electricity it was natural to find work continued in Maxwell's laboratory. J. J. Thomson had, in a paper in the *Phil. Mag.* 1881, considered some of the effects produced by the motion of the medium which is the seat of electrostatic action, assuming the electromagnetic theory of light. In 1885 he extended the earlier work from the case of translational motion to that of rotational, and found that the plane of polarisation after light has travelled any distance in a rotating medium would be rotated through an angle equal to that turned through in the time taken by light to traverse that distance. To twist the plane through 20″ in a distance of 10 metres, the medium would have to make about 500 revolutions per second.

Wilberforce (1885) made experiments with a delicate method of interference fringes, to see whether a displacement current of electricity involves a motion of translation of the electromagnetic medium. Thomson's work in 1880 had shown that if light is an electromagnetic disturbance in a moving medium, its velocity in a substance, which moves in the direction of propagation of the light, is increased by half the velocity of the substance, or rather half the velocity of the associated medium. Fizeau had found that the

change of velocity of light in moving water is about half the velocity of the water, and the inference is that water and the luminiferous medium move with the same velocity. Roiti had failed in 1873 to find any change of velocity when an electric current was flowing in a solution of zinc sulphate. Lecher in 1884 also failed to find an effect when a current of six amperes was passed through silver nitrate. Wilberforce failed to find any shift of his interference fringes when displacement currents of 0·2 micro-amperes passed through glass. Lord Rayleigh in 1888 failed to detect a change in the velocity of light in an electrolytic liquid carrying a continuous current of 1·5 amperes (current density 0·26 ampere/ sq. cm.) with a method that would have disclosed a change of one part in thirteen millions.[1]

Other researches bearing upon the truth of the electromagnetic theory of light were those of Cassie. That quality of the medium between two electrified bodies, in virtue of which the force between the bodies is changed when the medium is changed, is called the specific inductive capacity. In the electromagnetic theory of light, the refractive index μ is found to be related to the specific inductive capacity K, so that $\mu^2 \propto$ K. But in many methods of determining K it is measured for steady forces, or for such as are reversed only a few thousand times a second. Thomson (1889) devised a method which he applied to determining K when the force is reversed 25,000,000 times a second, by electrical oscillations, and showed that for glass the value of K is very much less, when high frequency is used: and whilst with low frequency the relation $\mu^2 \propto$ K is not fulfilled, it is shown to be true for frequencies of the order of twenty million per second. Ebonite and sulphur on the other hand show small differences by the two methods.

Cassie (1889) studied the effect of temperature on the

[1] Collected Papers, III. 213.

specific inductive capacity of several dielectrics, and was able to show that for several transparent liquid dielectrics the effects are of the same order of magnitude as is indicated by the electromagnetic theory of light. This theory suggests that the coefficient for the specific inductive capacity should be twice as great as for the refractive index. Cassie found that the coefficient was negative in each case, but the ratio was not 1 : 2, but for the refractive index he used the results of Dale and Gladstone. Later (1891), having thought that the discrepancies might be due to differences between his specimens and those used by Dale and Gladstone, he made further experiments to measure the rate of change of refractive index for the same specimens of the liquids (Turpentine, Carbon bisulphide, Glycerine, Benzoline, Benzene, Paraffin) as were used in the electrical experiments. The values of the inverse ratio for these liquids was found to be 4, 6, 30, 2, 3 and—? Cassie left Cambridge in 1893 to become Professor of Physics at the Royal Holloway College and died in 1908.

In 1883 Thomson had made a determination of 'v,' the ratio of the electromagnetic unit of electricity to the electrostatic unit. The value then obtained ($2{\cdot}963 \times 10^{10}$) was considerably smaller than those found in several recent experiments, and accordingly the Professor repeated the measurements with Searle in 1890. After a good deal of trouble discrepancies were traced to the failure of a guard ring in a condenser to produce its full effect. This source of error was rectified, and a rotating commutator was used instead of a vibrating tuning fork, and the results of the new measurements showed much greater consistency between observed and calculated effects. The ratio 'v' was found to be $2{\cdot}9958 \times 10^{10}$ based on values of capacity determined with different frequencies of charging which ranged between 80 and 16 per second. Thus an outstanding discrepancy

was got rid of and the value of 'v' was shown to be in good agreement with the velocity of light.

Another research, made in 1886, with the intention of finding a crucial test of the truth of Maxwell's electromagnetic theory by attacking the vector potential through an experimental investigation of the question of the continuity of displacement currents in dielectrics, was started by Professor Thomson with myself. But Hertz's work in 1887 rendered the research unnecessary though it was carried far enough to show its extreme difficulty. In the preliminary work, which involved the production of a very strong magnetic field, it was found that we were dealing with magnetic inductions very much larger than any recorded at that time. Thus a magnetic induction of 28,000 was obtained when the magnetising force was about 1200 c.g.s. units. Rowland had given 17,500 as the probable maximum attainable for iron. Ewing was found to be working at the same subject, having reached inductions as high as 33,000, and accordingly the research was not pursued further. A short note was, however, published giving results of experiments on the magnetisation of iron rods, especially on the effect of narrow crevasses at right angles to their length.

Properties of Matter &c.—In the category of researches on the properties of matter, Chree followed out in 1889 a line of thought suggested by Thomson's 'Applications of Dynamics.' Joule had shown long ago that an iron rod increases in length when magnetised in a comparatively weak field. Bidwell showed later that when the field was strong enough the length became less than when there was no field. Moreover, Bidwell proved that cobalt in complete contrast with iron shortens when magnetised in weak fields and lengthens in very strong fields.

Now Villari's observations and also Sir W. Thomson's,

showed that when iron is subjected to a cycle of loadings and unloadings in a magnetic field there is a cyclic change in the magnetisation. The maximum magnetisation synchronises with the loaded or unloaded state according as the magnetic field is weaker or stronger than a certain critical field depending on the load. This has been called by Sir W. Thomson Villari's critical field.

Here then is an opening for the application of J. J. Thomson's dynamical principles, if indeed these phenomena are controlled by such principles. Has cobalt a Villarian critical field? If so, this is a point gained for dynamics. If not, we must be content to think that the behaviour of matter is ordained rather by a Puck, who leads the wretched experimenter 'up and down, up and down.' Chree's experiments decided in favour of the result foreshadowed by Thomson's theory.

Thomson's work on vortex theory (his essay had gained the Adams Prize in 1881) led him to see an explanation for the formation of vortex rings when drops of liquid drop from not too great a height into a vessel containing some of the liquid at rest. He and I (1885) examined the case experimentally to test the truth of the theory, and we succeeded in showing that, when a drop falls into a viscous liquid ocean, a necessary antecedent for the formation of a vortex ring is that the drop should be flattened in its descent into a disc-like body; such a body when it is filled with vorticular motion is unstable, according to Thomson's theory, and it passes into the form of an anchor-ring, which is stable. Sundry effects of viscosity were also studied, as also the effects of surface tension between the drop and the ocean.

The viscosity of liquids forms the subject of two other papers. Wilberforce (1891) discussed the appropriateness of certain corrections which should be made in calculating

the coefficient of viscosity from observations of the rate of flow through a fine tube of known small dimensions. In his paper he shows that account should be taken of the difference of character of the motion in the tube at points near its ends from that at points remote from them. He treated the earlier observations of Hagenbach (1860) with a new correction, and showed the improvement resulting in the deduced coefficient.

Whetham (1890) was led to look into the question of slipping at the boundary of a liquid in motion past a solid. The earlier observations of Helmholtz and Piotrowski pointed to the existence of a slip when a metal sphere filled with liquid was set oscillating about a diameter. If the coefficient of slipping deduced by them was real, then theory would indicate that water would take twenty times as long to flow through a glass tube as through a gilt tube of the same length and diameter. Whetham's observations showed that there was no evidence of slipping in silvered tubes, either when the velocity was such as to give linear motion or when the motion was pushed near to the limits of turbulent motion. His results showed that no slip occurs at any rate with solids that are wetted by liquids.

In 1886 Leahy spent some considerable time in working with the apparatus with which Maxwell experimented on the viscosity of gases. Some recent experiments, whilst corroborating the laws which Maxwell established both theoretically and experimentally, viz. the dependence of viscosity on temperature and independence of pressure, showed that the absolute value which Maxwell deduced was too high. For instance Tomlinson's results differed by about 9 per cent. from Maxwell's. Leahy pointed out (Maxwell's Papers, errata to Vol. II.) that in finding the value of the quantity A Maxwell employed a value of the radius of the oscillating disc different from that previously

recorded. A was given as 1112·8 and should have been 1220·8. If this is changed Maxwell's result is reduced so as to approximate to values obtained by recent experimenters. Stokes (Papers, Vol. V. 181), in discussing Tomlinson's results, thought that Maxwell's work might be in error because the discs were not level. But Leahy found that this error would not be sensible in Maxwell's apparatus.

The only other work relating to the kinetic theory of gases was that of Capstick on the specific heats of compound gases which was carried out during this period (1894). Maxwell in his development of the kinetic theory of gases discussed the consequences that would result if the particles were not spherical. The collisions would in many cases set up a motion of rotation as well as a change in the velocity of translation in individual molecules. The energy of the moving molecules would accordingly be partly rotational, partly translational. If the ratio of the total energy to the energy of translation is represented by β, we may write

$$\text{whole energy} = \beta \times \text{translational},$$

and if γ represents the ratio of the specific heat of a gas or vapour at constant pressure to the specific heat at constant volume, it may be shown that

$$\beta = \frac{2}{3} \cdot \frac{1}{\gamma - 1} \text{ or } \gamma = 1 + \frac{2}{3\beta}$$

If n represents the number of degrees of freedom other than those of translation, it may be shown that

$$\gamma = 1 + \frac{2}{n+3}$$

and if n is zero the whole energy is translational.

Corresponding values of γ, β and n are shown thus:

γ	=	1.0	1.1	1.2	1.33	1.40	1.5	1.60	1.66	2.0
β	=	∞	6.66	3.33	2.00	1.66	1.33	1.11	1.00	0.66
n					3	2	1		0	

It was known that for many so called fixed gases such as nitrogen, air, hydrogen and also for HCl, HBr, HI, the value of γ differs very slightly from 1.40.

Maxwell showed that for hard smooth spheres β should be 2. Bryan showed in 1894 that the equipartition of energy would fail for molecules that have an axis of symmetry ($n=2$). Naumann in 1867 suggested that n represented the number of atoms. J. J. Thomson (1890, in 'Watts' Dict. of Chem.,' Vol. 1, p. 89, 'States of Aggregation') suggests that for molecules of a certain kind of symmetry $n+3$ may be proportional to the number of atoms in a molecule.

Capstick's experiments aimed at finding something definite in this difficult and interesting subject by studying the changes in γ, the ratio of the specific heats of vapours, by Kundt's method of measuring the velocity of sound in the vapours. There were two researches; in his first (1893) he dealt with the vapours of the paraffins, methane (CH_4), ethane (C_2H_6), and propane (C_3H_8) and their derivatives, and he found that the monohalogen derivatives of any one paraffin have in the gaseous state the same ratio of specific heats, and that this ratio is the same as that of the hydrocarbon itself in the cases of ethane and propane. But methane is an exception, having a higher ratio than its derivatives. In his second research (1895) he found that the conclusion about the paraffins holds good for unsaturated hydrocarbons and their monohalogen derivatives. He found too that the addition of a second atom of Cl to a paraffin causes a large fall in the value of γ whether the first has done so or not.

Capstick points out that Strecher's observations proved that the ratio of specific heats

for H_2	is	1·40
for HCl, HBr, HI, BrI, and ClI	is	1·40
and for Cl_2, Br_2, I_2,		1·3

thus the substitution of one halogen in the hydrogen molecule does not change γ; and the introduction of the second halogen causes a large fall. But here all trace of the hydrogen molecule is gone. Capstick finds for the methane group of derivatives

		γ	$n+3$
CH_4	Methane	1·313	6·4
CH_3Cl	Methyl Chloride	1·279	7·2
CH_2Cl_2	Methylene Chloride	1·219	9·0
$CHCl_3$	Chloroform	1·154	13·0
CCl_4	Carbon Tetrachloride	1·130	15·4

Jeans, in his 'Dynamical Theory of Gases' (1904), regards Capstick's experiments as proving conclusively that no general law can be expected to relate γ with n, independently of the nature of the atoms. He suggests that non-integral values of n may be connected with dissociation.

My own work on the recalescence of iron was begun about 1887, but after the publication of three notes on the general phenomena Hopkinson's researches intervened, and the experiments were discontinued. The mention of them, however, serves to bring to my memory the stimulating effects of Lord Kelvin's visits to the Laboratory. He seldom came to Cambridge without visiting the Laboratory, and the interest he took in all the work that was going forward seemed to give it a new value. It was in one of these visits that he passed through the room where I was studying the changes in magnetic susceptibility

at the recalescence point, and I was able to show him the phenomenon of recalescence in a piece of thin steel sheet such as is used for making pens. When such a sheet is held stationary in the hot part of a blowpipe flame, incandescence in the sheet spreads out gradually from the centre of heating, and when the blast is cut off the recalescence shows itself in the form of a bright ring which slowly contracts upon the centre; the inverse phenomenon may also be seen in the form of a dark ring which expands from the centre as heat is supplied at that centre. The phenomenon was new to Lord Kelvin, and I recall the eager way in which he adjusted his eyeglass and moved two or three short steps nearer to the flame to study the effects, exclaiming several times 'Do that again!'

Worthington, who is now Professor of Physics at the Royal Naval College, Greenwich, carried out some interesting and difficult experiments on the mechanical stretching of liquids (1888). It can hardly have escaped the attention of anyone who has carefully constructed a mercury barometer, that sometimes the mercury clings to the top of the tube, so that the column is considerably higher than that required to balance atmospheric pressure. The mercury above the barometric height is, in fact, in tension. With care a column of many times the barometric height may be held up in this way. Osborne Reynolds studied the phenomena by using a centrifugal method in which a U tube filled partly with air-free liquid and partly with its vapour is whirled about an axis. Berthelot used a third method, which may be called the method of cooling; air-free liquid was sealed in a glass tube, the quantity being adjusted so that at ordinary temperatures the small residual space in the tube was filled with air-free vapour; when the tube and its contents were sufficiently heated, the liquid expanded and filled the tube completely. On cooling, the liquid

seemed to prefer to cling to the walls of the tube rather than allow vapour to be formed, nor would it let go till special means were employed. Worthington saw that the methods hitherto used would give measures of either the stress or the strain (extension), but not both. And having made measures of the stress by Osborne Reynolds's method he devised a method by which he was able to estimate both stress and extension. In his early experiments he used to attach his U tube to the lathe in the small room between the battery room and the present workshop of the Laboratory. When it is stated that it was a question whether the glass vessel or the liquid gave way first or both simultaneously, and that he succeeded in reaching a tension of 7·9 atmospheres with alcohol, and 11·8 atmospheres with strong sulphuric acid, it will be understood that there was some hesitation about accepting Worthington's invitation to help him to corroborate observations. These observations gave only a measure of stress. In the apparatus he devised for getting both stress and strain, there was constant danger that the suddenness of release may break the whole apparatus. There was also great difficulty in avoiding the liberation of air at some place in the vessel where adhesion was incomplete; in such cases the full tension was not available. But Worthington carried his work to a successful issue, and found that the changes of volume are equal for numerically equal increments of pressure *whether positive or negative*, and the coefficient of extensibility remains constant up to tensions of 17 atmospheres.

Skinner experimented on the compressibility of liquids (1891), and found the values for water in satisfactory agreement with Tait's extrapolation formula. He aimed at comparing the compressibility of solutions with that of solvents. His results exhibited the same general relations as the other properties of solutions. Solutions of electrolytic

type are considerably less compressible (3 per cent. solution of NaCl less by 8 per cent.). Non-electrolytic solutions show very small (if any) diminution. This is in agreement with J. J. Thomson's interpretation of osmotic pressure, and also falls in with the observed alteration in boiling or freezing points.

Heat and Thermometry.—In the domain of heat the chief work to be referred to is that of Callendar on the measurement of temperature. At that time Siemens' pyrometer was under a cloud, for the report of the British Association Committee in 1874 had shown that it was liable to large and continuous changes of zero. In the form in which it was then made it consisted of a platinum wire wound on common clay and inclosed in an iron tube without sufficient protection from vapours which are liable to attack it when it is used at high temperatures. The measurement of high temperatures with some trustworthy method was a great desideratum, and Callendar has stated how he was encouraged by the suggestions of Professor Thomson that he should attempt to make standards which could be issued in the form of platinum wire, the change of electrical resistance with temperature being determined by comparison for each specimen before issuing. Thereupon (1887), in 'the home of standards,' he began that series of refined observations which resulted in the establishment of platinum thermometry. Week after week one saw Callendar at work in the passage room on the ground floor of the Laboratory. There was a beauty of finish and simplicity about his apparatus that always disposed one to spend some minutes each day in watching his progress. After about a year's work he had shown that a platinum resistance thermometer sufficiently protected from strain and contamination was practically free from changes of zero over a range of 0° to

1200° C., and satisfied the fundamental criterion of giving always the same indication at the same temperature. Siemens' idea was shown to be right; it was the practical application that was at fault. Callendar went on to show that the properties of platinum were definite enough to make the issue of tested standards unnecessary. Even purity of the metal is not an essential, provided the specimen is protected from strain and contamination. For Callendar found that different specimens of platinum wires wound side by side, and wound in such a way as to be always at the same temperature, agreed in giving the same value of temperature, although they differed considerably in the values of their temperature coefficients. Callendar, therefore, proposed that the platinum thermometer should be used as a secondary standard, the temperature t_p on the platinum scale (called platinum temperature for short) being defined by

$$t_p = 100\ (R - R_0)/(R^1 - R_0),$$

where R, R_0 and R^1 represent the observed resistances at the temperature t_p, 0° and 100° C. respectively. He then showed by direct comparison of the platinum scale and the scale of the air thermometer that the deviations of the platinum scale from the temperature t by air thermometer could be represented by the simple difference formula

$$D = t - t_p = d \times \left(\frac{t}{100} - 1\right)\left(\frac{t}{100}\right)$$

with a probable error of less than 1° C. over a range 0° to 650° C.

In 1890 Griffiths found disconcerting discrepancies between his own results and Callendar's. But he and Callendar were able to join forces in tracking them to their source; and the discrepancies were traced to the fact

that Griffiths had assumed Regnault's value for the boiling point of sulphur. Harmony was thus attained, and the measurement of temperature is now on an entirely different footing, whether it be a matter of measuring infinitesimally small differences of temperature, or estimating high and low temperatures far outside the limits at which mercury thermometers become completely useless.

There were moments in the research when we were in doubt whether it was thermometry or shorthand that was to carry the day. For Callendar devised a new form of shorthand writing, and published a book about it. In a report on thermometry which he wrote in 1899, there is a characteristic sentence which recalls to my mind his 'cursive shorthand.' He is speaking of notation, and he says: 'In devising the notation special attention has been paid to the limitations of the commercial typewriter, as the majority of communications . . . are required to be type-written.' It is characteristic of him that he should not be content with developing a scientific method without paying attention to practical points on which success so often depends.

The only other researches that come under the heading 'Heat' in this period are those of Chree (1887) on the 'Conduction of Heat in Liquids.' There were discrepancies between the values obtained by Bottomley and Guthrie in this country and those obtained more recently in Germany by Weber and others. In recent work measures had been made with layers of liquid so thin that the conditions at the surfaces limiting the liquid layer are of undue importance. Chree therefore decided to use a method which avoided this source of uncertainty. The experimental liquid was poured into a wooden tub, in the top of which was supported a thin metallic disc or tray. In the liquid, at some distance below the tray, a platinum wire was stretched

horizontally and connected with a galvanometer &c. in a way that served to give indications of the change of temperature of the wire. Hot water was poured into the tray, and the interval that elapsed before the platinum wire was being most rapidly heated was noted. This work was carried out in 1886–7.

Electricity and Magnetism.—In electricity and magnetism, apart from the work that was carried on in connexion with electrical standards and has been dealt with in Chapter IV, the principal papers may be separated into three categories: those relating to electrolysis, those relating to isolated phenomena, and those connected with the conduction of electricity in gases.

The second category includes Wilberforce's experiments (1886) on specific inductive capacity, wherein he used spheroids of dielectrics and of conducting material for comparison. The older methods depended on the use of spheres and the measurement of a force. Wilberforce adopted the easier method of measuring directive couples on the spheroids. McConnel in 1886 dealt with the proof of Maxwell's expression for the mechanical force acting on an element of a magnet carrying a current. Monckman (1888) studied experimentally the distribution of a number of electrified cylinders parallel to each other when acted on by an electrified sphere placed above them, and compared the results with Mayer's experiment with floating magnets. Searle (1892) devised his ingenious compound magnetometer, which caused a spot of light to trace out a hysteresis curve on a screen. Emery (1894) studied the thermo-electric properties of solutions in order to test results previously obtained by Bouty.

The study of electrolysis attracted a great deal of

attention. There was a committee of the British Association actively at work. Shaw was engaged upon a report on the whole subject, and there were many points requiring elucidation. Thus Fitzpatrick (1886) was studying the application of alternating currents to the determination of the conductivity of electrolytes, in extension of the earlier work of Kohlrausch and Nippoldt, who had found that above a certain frequency an electrolytic cell could be replaced by a metallic resistance, the value of the resistance remaining constant for higher frequencies. Fitzpatrick showed that his method was trustworthy in respect to polarisation effects, induction effects, and effects of contact resistance; and proceeded to study (1887) the action of the solvent in electrolytic conduction.

Coldridge (1890) experimented on stannic chloride, a liquid which Faraday found to be a non-conductor. He showed that though the salt could absorb dry HCl gas and thereby attain very slight conducting power and exhibit polarisation effects, chloroform could be mixed with it without producing any conducting power. When the tin chloride absorbs H_2S, a yellow liquid is produced without a trace of conducting power; but the addition of a few drops of water or alcohol at once precipitates tin sulphide and produces conducting power in the liquid.

The situation was much enlivened by the new form that Arrhenius gave to the Clausius-Williamson hypothesis of the dynamical equilibrium of a chemical compound, connected with the continual dissociation and re-formation of molecules. Shaw's 'Report on the Present State of our Knowledge in Electrolysis and Electrochemistry' to the British Association in 1890 shows how big an advance was being made at that time.

Williamson (1851) showed that the facts of sundry chemical actions require us to suppose that there is a continual molecular interchange of partners going on. Clausius

in 1857 pointed out that the same idea is necessary to the understanding of electrolysis.

Hittorff's work from 1853 to 1859 explained the facts of the observed changes of concentration round the electrodes by supposing that there was continual migration of ions and that the velocities of different ions were different. Kohlrausch from 1869 to 1874 showed that by dealing with dilute solutions we arrive at the idea of specific ionic velocities; for in such solutions the velocity of an ion, *e.g.* of Ba, tends to a limiting value which is independent of the nature of its partner in combination. In fact, we see that water can be prepared with extremely high insulating power; and in very dilute solutions, the addition of a small quantity of salt confers proportionate increase of conductivity; thus water molecules do not contribute to the conducting powers which are due to the salt. Conductivity is thus proportional to concentration, and the magnitude of the increased current depends on the increased number of ions conveying it, not on changes in their velocity. If ions are not permanently free from one another, they must travel by interchange from partner to partner, but since the velocity of travel must increase if the number of molecular interchanges becomes more frequent, that is, if the concentration increases, Kohlrausch's discovery of unaltered velocity of travel points to dissociation. This was Arrhenius's view. Kohlrausch's deduction rests on the truth of Ohm's Law in electrolytes, but he had proved that, if polarisation is eliminated, the resistance of a liquid is constant, and thus he established Ohm's Law for electrolytes. His proof was corroborated by Fitzpatrick's work and also by the work of Fitzgerald and Trouton. Ohm's Law means that no work is done in overcoming reversible E.M.F.s like those of polarisation. Thus the molecular exchange of partners must go on whether the current flows or not. All that the applied

E.M.F. does is to direct the electrified ions in opposite directions, and if it is big enough to overcome the polarisation at the electrodes it causes a continued evolution of products of electrolysis.

Whetham's work on the velocity of ions in electrolysis was carried out in 1890-4. Lodge had in 1886 described experiments that made visible to the eye the motion of the ions in electrolysis, Kohlrausch's specific ionic velocities having been derived from theoretical considerations, which, though based on experiment and observation, did not make the motion directly visible. Lodge used an electrolytic cell of baric chloride solution, divided into two parts, which were joined by a tube filled with a conducting jelly impregnated with a solution of sulphate of silver. The barium ion travels with the current and leaves a precipitate of $BaSO_4$ in the jelly, whilst the Cl travels against the current and leaves a precipitate of AgCl in the jelly. The relative velocities of the ions is thus actually seen in the gradual creep of the two precipitates towards one another along the tube of jelly. Whetham devised another method depending on the use of solutions of coloured salts. For example, solutions of potassium carbonate (colourless) and of potassium bichromate (orange) are arranged of the same molecular concentration and conductivity, so that the denser solution rests below the lighter one in a vertical tube and electrodes are inserted in the liquids in such a way as not to disturb the liquids. When the current passes across the junction between the liquids the colour boundary is seen to move, and from the rate at which it creeps along the tube the velocity of the bichromate ion (the acid part of the salt, to which the colour is due) is deduced. The velocities are of the order of about one centimetre per hour. Whetham's results show that the velocities thus deduced are in excellent agreement with those deduced by Kohlrausch from observation of the electric conductivity of solutions.

Scant justice has been done in these pages to a great deal of other work done in this period 1885-1894. Amongst the many mathematical papers come several papers by J. J. Thomson, Glazebrook's work on gratings and on quasi-labile ether, Chree's work on elasticity, Michell's work on the theory of stream lines, Schott's work on reflexion and refraction of light.

I have not referred to W. N. Shaw's work on hygrometry nor yet to his investigations which have made him an authority on ventilation. The first forms the subject of a memoir in the *Philosophical Transactions* of the Royal Society. The second are in part summarised in a volume published in 1907 and described by the author as his 'last will and testament in respect of ventilation.' The book is based on a course of lectures delivered at Cambridge, and the subject-matter rests in considerable degree upon the work which Shaw carried out with the assistance of R. S. Cole with the pneumatic analogue of Wheatstone's Bridge in 1889-90.

The Passage of Electricity through Gases.—Throughout all the work that I have been attempting to recall, Thomson was leading an attack on the processes involved in the conduction of electricity through gases. Maxwell had written in his treatise in 1872:

> These and many other phenomena of electrical discharge are exceedingly important, and when they are better understood they will probably throw great light on the nature of electricity as well as on the nature of gases and of the medium pervading space.

The attack seemed in some ways more difficult and less hopeful than the study of electrolysis in liquids. Yet the properties of gases and the gaseous laws are simpler than for those of solids or liquids. The kinetic theory, moreover,

allows us to form clearer mental pictures of what goes on in a gas.

Crookes's work on the trajectories of molecules in high vacua was published in 1879, and Goldstein's researches on 'that remarkable motion which radiates from the kathode in rarefied gases' had appeared in 1880 and was translated in the *Philosophical Magazine* in that year. In the same journal in 1881 there appeared J. J. Thomson's paper on the electric and magnetic effects produced by the motion of electrified bodies; in it he pointed out that in Crookes's and Goldstein's investigations matter highly charged with electricity and moving with great velocities form a prominent feature in the phenomena, and that a large portion of their work consists of experiments on the action of such particles on each other and their behaviour when under the influence of a magnet. He accordingly undertook the task (both by way of testing theory and for the purpose of guiding future experiments) of finding what is the force existing between two moving electrified bodies and in what way the bodies are affected by a magnet. He showed that the interaction between the electro-dynamic field and a moving electrified body involves an addition to the effective mass of the body depending on the velocity of movement; and he explained the green phosphorescence seen in highly exhausted vacuum tubes, referring it to the sudden stoppage of the kathode particles.

In 1883 J. J. Thomson published (*Phil. Mag.* xv. 432) a paper on the theory of the electric discharge in gases. He deals with vortex atoms, and accepting the view of Clausius and Williamson about the constant interchange of partners between molecules, he introduces the idea of the ratio between the paired time and the free time of atoms, and insists that, in the view he presents,

Chemical decomposition is not to be considered merely as an

accidental attendant on electrical discharge but as an essential feature of the discharge, without which it could not occur.

In 1885, when I returned to Cambridge as assistant to the Professor, he was engaged in search for signs of permanent dissociation produced by electric discharge through gases and vapours. He worked with Threlfall (1885) at the dissociation of iodine and bromine, and also (1886) at the changes produced in nitrogen by electric discharge. Again, with Threlfall in 1886, he worked at the production of ozone, and showed that a steady electric field would not convert oxygen into ozone unless discharge passed. Two foreign students were then working in the Laboratory. Natanson studied dissociation in gases like nitrogen peroxide. Olearski, at the Professor's suggestion, undertook some experiments (1885) on dielectric strength of mixed gases. The Professor himself studied electrical discharge in a uniform field (1886) in hope of being able to trace the state of the field before discharge takes place. At that time the distinction was not clearly drawn between the disruptive effects of sparks and the steady conduction of discharge. The approach to the position to be stormed seemed to lie through the strength of dielectrics.

In 1886–87 Thomson and I studied the leak of electricity through bad conductors, like benzene, paraffin oil, carbon bisulphide, and olive oil, and found that for small electromotive forces it obeyed Ohm's Law, which implied that no work was done by the current in overcoming reversible E.M.F.s like those of polarisation. Quincke had previously shown that for large E.M.F.s (of the order of 30,000 volts) the leak was proportional to a higher power than the first of the E.M.F. In the latter case the electric field has to split up the molecules; in the former it is simply a matter of rearrangement of some molecular condition which is not *produced* by the field.

Thomson proved in 1889 that if a permanent gas in a closed vessel be heated up to 300°C., the discharge potential does not change. If the vessel be open, so that the pressure remains constant, there is a diminution in the potential, and this is to be ascribed to the change in density accompanying the rise in temperature. If the temperature is so high that dissociation takes place, the discharge potential may fall very low, even to zero.

In the same year, 1889, Thomson studied the resistance of electrolytes for alternating currents of very high frequency, and found that electrolytes are as good conductors when the current is reversed a hundred million times a second as for a steady current. Electrolytes are transparent, and by Maxwell's theory they must act as insulators for currents reversed with the frequency (10^{15}) of light vibrations. Thomson infers that the molecular processes which produce electrolytic conduction must occupy a time between 10^{-8} and 10^{-15} second.

Then began Thomson's work on discharge through hot gases (1890), showing that they come under two categories—dissociable gases which become good conductors, obeying Ohm's Law: and non-dissociable gases which conduct but slightly. The importance of glowing terminals was discovered, as a means of getting the discharge to pass into a gas. Then followed (1891) the study of electrodeless discharge, wherein Thomson showed that when a vacuous bulb was surrounded by a few coils of wire, conveying the rapidly oscillating currents connected with the discharge of a condenser (Leyden jar), the gas in the bulb could be made to play the part of a secondary circuit of an induction coil, in which the primary circuit consisted of the few coils of wire outside the bulb. There was no question here of semi-electrostatic effects such as Tesla produced: it was a true electromagnetic phenomenon, involving a discharge

of electricity through gas accompanied by brilliant luminosity.

In the same general connexion of the influence of electrodes came Chree's experiments (1891) on liquid electrodes. This work showed, though the phenomena of the dark space at the negative electrode are not in general changed, the dark space is considerably shorter over an electrode of sulphuric acid than over aluminium.

In the work on electrodeless discharge, Thomson found that it was often exceedingly difficult to get the first discharge to pass, but when once this is accomplished (by bringing an electrified body near the bulb, or passing a brush discharge) the discharge passes readily. The change was ascribed to the setting free of dissociated atoms. Hittorff had made a similar observation with respect to ordinary vacuum tubes with electrodes, and Schuster deals with the case (he, too, had experimented on it) in his second Bakerian Lecture in 1890. Thomson, after many observations of extraordinary interest, laid aside the study of electrodeless discharges; the method presented difficulties in the way of quantitative measurement, the discharges being oscillatory and having very high frequency of alternation.

The attack through the electric strength of gases was also relinquished by degrees, as it became evident that this was as much dependent on the nature and condition of the electrodes used as upon any property of the gas. I am not quite clear when it was realised that the magnitudes involved proved that the electric forces could not produce dissociation. But in his lectures on discharge in gases (see also 'Recent Researches,' p. 193) the Professor put into the forefront the proof that the electric force was generally so much smaller than the force required to disrupt the oppositely electrified atoms in a molecule, that it could only be held to produce disruption of the few molecules which just failed to be disrupted

by collisions. At any rate, hope seemed to be relinquished of succeeding through study of electric strength. The matter was reverted to when Peace's observations on minimum strength (1892) showed the pressure for minimum strength depends on the length of the spark.

Thomson interpreted this as meaning that the scale of structure in a gas conveying electric discharge is more nearly comparable with the striæ in the discharge than with the diameter of a molecule, or the average separation of neighbouring molecules or even the mean free path. Peace's observations showed that minimum strength may occur at a pressure as high as 250 mm.; Thomson's observations (1891) showed that the electrodeless discharge passes with greatest ease at a pressure of about 1/250 mm. Thomson's inference was that the necessary coarseness of structure would be provided if the gas for these purposes was built up of short polarised trains or chains of molecules (Grothuss).

Thomson's work on the rate of propagation of the luminous discharge (1891) showed from observations made with a rotating mirror that the luminosity starts from the positive electrode and travels with a velocity which is rather more than half the velocity of light, a velocity which (in the proved absence of the Doppler effect) was interpreted as that of propagation of a disturbance consisting in an interchange of partners at the adjacent ends of a series of broken chains or trains of polarised molecules along the path of the discharge. [I believe I am right in saying that the term Grothuss chain which occurs so frequently in the 'Recent Researches' (1893) does not occur in the book on the 'Conduction of Electricity in Gases' (1903). I am also surprised to find that Grothuss's hypothesis dates back to 1808.]

In 1893 Thomson studied the effect of electrification and

chemical action on a steam jet and showed that negative electrification is more effective in producing condensation than positive electrification. (C. T. R. Wilson reverted to this point in 1899.) Thomson points out that 'when aggregates are formed round nuclei, discharge through a gas may consist in tearing atoms off complex aggregates. In 1893, at the time when he was bringing out the third revised edition of Maxwell's 'Treatise on Electricity and Magnetism,' together with the third volume 'Notes on Recent Researches,' Thomson repeated and extended Perrot's experiments (1861) on the electrolysis of steam, and showed how the products of electrolysis depend upon the length of the spark used. In the following year he studied the electrification of drops, which offer many advantages for the investigation of electrical effects, especially of those which involve the contact of dissimilar substances.

In the next year, 1894, Thomson made a determination of the velocity of kathode rays, judged by the rate of propagation of luminosity from the kathode, and found a value 1.9×10^7 cm/sec, much greater than the velocity of the mean square of molecules (1.8×10^5 cm/sec for hydrogen at 0° C.) and much less than that of the main discharge from the positive to the negative electrode (1.6×10^{10} cm/sec). The measurement was made by means of a rotating mirror, and the velocity thus found was shown to agree very nearly with the velocity which a negatively electrified atom of hydrogen would acquire under the influence of the fall of potential near the kathode, if it were charged with an electrolytic charge so that $e/m = 10^4$. It was also pointed out that the deflection of the kathode rays in a magnetic field shows that the velocity of the charged atom must be at least of the order found. The paper closes with an estimate of the magnetic field required to produce a curvature $\rho = 10$ cms.: and H is found to be 200, if $v = 2 \times 10^7$. The

final clue was thus almost within Thomson's grasp in 1893: he did not actually get it till 1897.

I have endeavoured in this condensed *résumé* to indicate the several steps from the point of view of the knowledge at the time, without forestalling what was to come in 1897 after the discovery of Röntgen rays and Becquerel rays. The advance continued step by step, now by one approach, now by another. Well nigh hopeless must be any attempt to summarise in a few pages the lines of thought, as they appeared to an onlooker, in Thomson's work which has contributed in such large measure to the advance of our knowledge of the subject.

As a sign of the activity in this branch, it may be recalled that there were no less than three Bakerian Lectures devoted to electric discharge in gases between 1884 and 1890. Schuster's first lecture in 1884 called attention to the peculiarities of the discharge in mercury vapour, which is held to be a monatomic gas. He could express a belief that the process of discharge was electrolytic in nature and be strengthened in the belief by Thomson's having expressed his faith in it in 1883. Thomson in his Bakerian Lecture in 1887 dealt with attempts to prove that permanent dissociation can be produced by electrolytic action in gases.

In 1890 Schuster gave his second Bakerian Lecture, emphasising his belief that the want of symmetry between the phenomena at the positive and negative electrodes would give the most important clue to the interpretation of the processes of discharge. And now that we know the later history, it is with sympathetic interest that we read how tantalisingly near Schuster came to the discovery of the central fact, when he deduced the ratio e/m for the kathode particles, *assuming the velocity of the mean square for the particles*, instead of finding means of making an independent determination of the actual velocities. Schuster

measured the curvature of the jet of particles in a known magnetic field, and assumed a velocity in agreement with our knowledge at the time. Thomson in 1893 measured the velocity, and calculated the magnitude of the magnetic field that would be wanted to produce a measurable deflection of the jet of particles. It was not till 1897 that Thomson got the real clue by experimentally unbending by means of an electric field the curvature imparted by a magnetic field to the stream of kathode particles, and so deduced the true e/m for the kathode particles or corpuscles, and thence deducing their velocity and mass.

But this later development must be left to the writer of the next chapter.

CHAPTER VI

(1895–1898)

THE short period under review (1895 to 1898) is one of the most interesting and important in the history of research in the Cavendish Laboratory. It is remarkable not only for the number of new discoveries of the first importance, but also for the inception of those newer ideas of the connexion between electricity and matter which have so greatly influenced the trend of research in the Cavendish Laboratory during the last decade. The years under consideration synchronise with that period of intense activity in Physical Science which had its beginning with the experiments of Lenard on the Cathode Rays and the discovery of the x-rays by Röntgen in 1895. It was essentially a period of pioneer advance into a new and fertile territory, when new ground was broken day by day and when discovery after discovery followed in quick succession.

In this rapid advance the Cavendish Laboratory played a notable part. Amongst other discoveries it witnessed within its walls the final proof of the nature of the cathode rays, the advent of the negative corpuscle, or electron, as a definite entity, the experimental proof of the character of the conduction of electricity through gases, and the initial analysis of the radiations from radioactive matter.

There is no doubt that the sudden development of the activities of the Laboratory in research was due in a

great measure to the new policy of the University, whereby 'advanced students' who had graduated at other institutions were admitted to membership and to degrees without passing through the normal undergraduate course (see p. 91). Of the following workers in the Laboratory in the period under consideration, C. T. R. Wilson, E. B. H. Wade, S. J. Smith, J. S. Townsend, E. Rutherford, J. McClelland, J. Erskine Murray, P. Langevin, W. Craig-Henderson, J. Henry, J. Zeleny, V. Novak, G. B. Bryan, J. H. Vincent, L. Blaikie, J. McLennan, G. Shakespear, R. S. Willows, J. B. B. Burke, G. W. Walker, J. G. Leathem, R. G. K. Lempfert, less than half had entered the University as undergraduates.

Before discussing in detail the work of this period, it may be of interest to review briefly the state of knowledge of some of the important problems which were then on the verge of solution. We shall consider first the attitude of the more advanced scientific workers with regard to the nature of the conduction of electricity through gases. The beautiful and extraordinarily varied phenomena exhibited during the passage of an electric discharge through a gas at different pressures had early attracted a great deal of attention. Maxwell referred to this subject in a prophetic passage in his 'Electricity and Magnetism' (First Edition, p. 58).

> These, and many other phenomena of electrical discharge, are exceedingly important, and when they are better understood they will probably throw great light on the nature of electricity as well as on the nature of gases and of the medium pervading space. At present, however, they must be considered outside the domain of the mathematical theory of electricity.

It was early recognised that a gas was a perfect insulator of electricity provided the voltage applied was well below that required for a disruptive discharge. This indicated that the molecules of a gas could not be charged by

From a Photo by Stearn, Cambridge]

RESEARCH STUDENTS, 1899 :—

S. W. RICHARDSON J. HENRY

E. B. H. WADE G. A. SHAKESPEAR C. T. R. WILSON E. RUTHERFORD W. CRAIG HENDERSON J. H. VINCENT G. B. BRYAN

J. McCLELLAND C. CHILD P. LANGEVIN J. J. THOMSON J. ZELENY R. S. WILLOWS H. A. WILSON J. S. TOWNSEND

mere contact with an electrified surface. This view was supported by the observation that the vapour arising from a strongly electrified liquid was itself entirely free from charge. The conception thus arose that a gas could only conduct electricity when the molecules were in some way dissociated either by the action of a strong electric field or by other agencies.

The passage of electricity through a gas was ascribed to a transference by charged carriers, and in that respect resembled the conduction of electricity through liquids. This view was held by Schuster,[1] who made a number of experiments to obtain evidence on the question. He showed that the whole gas in a vessel was made conducting by passing an electric discharge through one part of it. For example, if the discharge tube was divided into two compartments, the passage of a discharge in one compartment produced a discharge of an electroscope in the other, provided always there was a free gaseous connexion between the two. This indicated that the charged carriers or ions were able to move rapidly through the gas at low pressures outside the main line of discharge. He also found evidence that the negative ion was more mobile than the positive, a result which was later independently observed in different experiments by J. J. Thomson (1895).

It was at this stage natural to suppose that the charged ions produced in gases were identical with the atoms of the dissociated molecules. In the electric field, the positive charged atom travelled to the negative electrode and *vice versa*. If the conduction of electricity were due to a type of electrolysis, the products of the decomposition of the gas should appear at the electrodes. This side of the question

[1] Bakerian Lecture: 'Experiments on the Discharge of Electricity through Gases,' *Roy. Soc.*, vol. xxxvii., p. 317 (1884); *Proc. Roy. Soc.*, vol. xlii., p. 372 (1887); Bakerian Lecture, *Proc. Roy. Soc.* (1890).

was vigorously attacked by J. J. Thomson (1893). He examined the gases appearing at the electrodes after a series of strong discharges had been passed through superheated steam. When the spark-length due to the discharge exceeded a certain limit, hydrogen appeared at the negative electrode and oxygen at the anode in the proportions to be expected from Faraday's laws of electrolysis of solutions. For spark-lengths below a certain value the conditions were reversed, the hydrogen appearing at the positive electrode and the oxygen at the negative; while for intermediate spark-lengths the results were very conflicting and irregular.

He then turned his attention to the discharge through vacuum tubes containing complex gases at low pressure (1895). In this case the spectroscope was used to determine the products of decomposition of the gas appearing at the electrodes. For example, when the discharge was passed through hydrochloric acid gas, hydrogen first appeared at the cathode and chlorine at the anode, but the conditions changed after the discharge had passed for some time. A number of interesting effects of the discharge in various gases were noted. While there was direct evidence of electrolysis in the passage of electricity through gases, it was difficult to show whether this electrolysis was a primary or secondary effect. No very definite conclusions could be drawn from the results as to the nature of the conduction of electricity through gases.

There was another interesting point which was carefully examined. If the view be taken that the hydrogen molecule is dissociated by the discharge, on passing a discharge through pure hydrogen, the positive atoms should be in excess at the negative electrode and the negative atoms at the positive. It was of interest to examine whether such oppositely charged atoms differed sufficiently to give different spectra. The only difference observed in the spectra

at the two electrodes was an alteration of the relative brightness of some of the hydrogen lines.

The phenomena occurring during an electric discharge through a vacuum tube are so complicated that even to-day, although we have much more definite knowledge of the nature of the processes taking place, it is still very difficult to interpret many of the phenomena observed during the discharge. In order to obtain a clear idea of the mechanism of conduction in gases it was necessary to await the discovery of agencies that make the gas a conductor of electricity without the necessity of applying a strong electric field, for the use of the latter enormously complicates the problem. This want was supplied by the discovery of the x-rays and the rays from radioactive matter. The way in which the advent of these radiations has thrown light on the general question will be discussed later in this chapter.

We must now digress for a moment to say a few words upon the rise of a new idea of the nature of electricity which has exercised a profound influence on the development of modern physics. Following the classical experiments of Faraday on the electrolysis of solutions, the view that electricity, like matter, was atomic in structure was suggested by Maxwell and afterwards by Weber. This point of view received strong support from the authority of Helmholtz, who put it forward clearly in a lecture before the Royal Institution in 1887. Electricity, like matter, was supposed to be not infinitely divisible, but there was a definite unit or atom of electricity. The charge carried by any body was either equal to this unit charge or an integral multiple of it. This point of view gave a simple and rational explanation of the facts of electrolysis. Taking the charge carried by the hydrogen atom as the fundamental unit of electricity, the charge carried by the oxygen atom, for example, in electrolysis was two units, by the gold atom

three units, and so on. From the data of the electrolysis of water, it was simply deduced that the value $\frac{e}{m}$, the ratio of the charge to the mass of the hydrogen atom, was 9660 electro-magnetic units.

Schuster[1] in 1884 saw that, if the cathode rays consisted of charged particles in motion, it was possible to determine the value of the magnitude $\frac{e}{m}$ for the particles, and thus throw some light on the question of the mass of the charged particles compared with the mass of the hydrogen atom.

When charged particles travel at right angles to a magnetic field of strength H, they describe a circular orbit of radius ρ, which is connected with the velocity u and the value of $\frac{e}{m}$ for the particle by the relation $H\rho = \frac{mu}{e}$.

In this equation both $\frac{e}{m}$ and u are unknown, but another equation can be obtained between these two quantities by supposing that the particle moves freely under the influence of the electric field applied to the discharge tube. If V be the difference of potential, the kinetic energy $\frac{1}{2}mu^2$ acquired by the particle is equal to Ve.

From a combination of these two equations, both $\frac{e}{m}$ and u can be determined. Schuster published some experiments made along these lines in 1890. The value of $\frac{e}{m}$ deduced from the above conceptions was in the neighbourhood of 10^7, or about a thousand times greater than the value of $\frac{e}{m}$ for the hydrogen atom. At that time, however, there

[1] Schuster, *Proc. Roy. Soc.*, 37, p. 317 (1884); 47, p. 526 (1890).

was no idea that the atom of electricity could exist independently of the atom of matter. Since the pressure of the gas in which the cathode rays were generated was not very low, it appeared probable to Schuster that the cathode particle must make a large number of collisions with the molecules in its path, and its actual velocity would be consequently very much less than that calculated on the assumption that it moved freely without resistance in the electric field. Making certain assumptions, he concluded that the results observed were not incompatible with the view that the cathode particle in air was a charged atom of oxygen or nitrogen. It is of interest to note that the method used by Schuster is identical in principle with that adopted later by Kaufmann, to determine accurately the value of $\frac{e}{m}$ for the cathode particle.

In the light of later knowledge, we now know that the retardation of the cathode particle in passing through the residual molecules of the gas in the discharge tube is much smaller than Schuster supposed. The time, however, was not ripe for the correct interpretation of the experimental results. Before this was possible, a still further increase of our knowledge of the cathode rays was necessary.

In the meantime Hertz took up the question of the nature of the cathode rays. He recognised that if the cathode rays were charged particles, they should be deflected in passing through an electric field. The experiment was tried, but with negative results, for reasons that will be mentioned later. He observed the very important fact that the cathode rays were able to pass through very thin films of matter.[1] This phase of the subject was taken up and greatly extended by Lenard.[2] By using a special discharge

[1] Hertz, *Wied Annal.*, 45, p. 28 (1892).
[2] *Wied Annal.*, 51, p. 225; 52, p. 23 (1894).

tube, he was able to pass the cathode rays through a very thin opening and study their properties outside the discharge tube. He made an extensive series of experiments on the properties of the rays and showed that their power of penetration of matter depended only upon the mass of matter present, and was independent to a large extent of its chemical composition.

These results were very remarkable and of the greatest importance.

At that time the idea of a type of radiation capable of penetrating matter opaque to ordinary light was quite new to science, while the remarkable law of absorption of the radiation by matter at once attracted attention. A short time later (November 1895) followed the culminating discovery by Röntgen of the *x*-rays. The extraordinary properties of these rays and their remarkable penetrating power created intense interest and immediately gave a great impetus to the study of the phenomena occurring in the passage of electricity through gases.

It was soon recognised that a close connexion existed between the cathode rays and the *x*-rays. In a focus tube, the cathode rays struck the anti-cathode and were accompanied by the production of the *x*-rays. In order to explain the cause and nature of the *x*-rays, it became of great importance to determine the character of the cathode rays themselves.

Immediately after the announcement of the discovery by Röntgen of the *x*-rays, Thomson (1896) showed that these rays made a gas at ordinary pressure a temporary conductor of electricity, and obtained strong evidence that the conductivity was due to the production of a number of positively and negatively charged ions throughout the volume of the gas. Thomson then made a close study of the cathode rays. If charged carriers could be produced in a gas at ordinary pressure by the *x*-rays, it appeared

probable that they would also be produced by the action of a strong electric field on a gas at low pressure. Consequently it seemed very probable that the cathode rays, as Crookes supposed, consisted of a stream of negatively charged particles. Before such a conclusion could be established it was necessary to show that the rays did carry with them a negative charge and that they were deflected by an electric as well as by a magnetic field.

In 1895, Perrin,[1] in an interesting experiment, had obtained evidence that the cathode rays carry with them a negative charge. Thomson (1897) repeated the experiment in a modified and more definite form and reached the same conclusion. He then made experiments to test whether a pencil of cathode rays was deflected in passing between two parallel plates which were kept at a constant difference of potential by means of a battery.

He found that at very low pressures of the gas the cathode stream was undoubtedly deflected towards the positive plate, as was to be expected if the cathode particles carried a negative charge. At higher pressures of the gas the deflection of the rays by an electric field was more difficult to observe. This was shown to be due to the marked conductivity or ionisation produced in the gas by the passage of the cathode rays. The movement of the gaseous ions in the electric field between the plates disturbed the potential gradient and greatly reduced the electric field near the centre of the plates. The disturbance due to conducting gas between the plates was thus effective in masking to a large extent the deflection of the rays. At low pressure the ionisation was much less and the electric field more uniform between the plates.

The existence of this effect at once explained the negative results obtained by Hertz in the experiments previously

[1] *Compt. Rend.*, 121, p. 1130 (1895).

referred to. The proof of the deflection of the cathode rays in an electric field was of great importance, for it added the final link to the chain of evidence in support of the corpuscular character of these rays.

It now became of great interest to form an idea of the mass of the negatively charged particles constituting the cathode stream. We have already seen that Schuster in 1890 had indicated a possible method of determining the magnitude $\frac{e}{m}$, the ratio of the charge carried by a particle to its mass, by measuring the deflection of the rays in a magnetic field and the voltage applied to the discharge tube. In making experiments of a similar character, Thomson was struck by the fact that the nature of $\frac{e}{m}$ deduced in this way for the cathode particle was always about a thousand times greater than the corresponding value for the hydrogen atom, even under conditions when the pressure of the gas in the discharge tube was so low that the cathode particle must make very few collisions with the gas molecules in its path.[1] Assuming the cathode particle carried the same charge as the hydrogen atom, this indicated that the mass of the cathode particle was very small compared with the mass of the hydrogen atom.

Thomson recognised that this form of experiment was not altogether free from objections, so he proceeded to devise other methods of determining the velocity of the cathode particle and the ratio $\frac{e}{m}$. A narrow pencil of cathode rays was produced in the discharge tube and its deflection measured in a known magnetic field. This gives the value of $\frac{m\,u}{e}$, where u is the velocity of the cathode

[1] An account of experiments of a similar kind was published by Wiechert in 1897.

particle, supposed constant. The narrow pencil of rays was allowed to fall on a small thermo-couple, and the heat communicated to it was determined. This gave a measure of the kinetic energy $\frac{1}{2}m\,n\,u^2$ of the rays, where n was the number of particles present in the discharge. Finally, the total charge $n\,e$ carried by the rays was determined by passing them into an insulated metal cylinder. From these three equations, the values of u and $\frac{e}{m}$ could be at once deduced.

Experiments were made with different discharge tubes containing air, hydrogen, and carbon dioxide at different pressures. Within the limit of experimental error, the value of $\frac{e}{m}$ was the same for all gases and equal to about 2×10^7. On the other hand, the velocity u was found to be variable depending upon the voltage necessary to produce the discharge. In most experiments the velocity of the cathode particles lay between 10^9 and 10^{10} cms. per second.

Another and simpler method of determining $\frac{e}{m}$, by noting the deflection of a pencil of cathode rays both in a magnetic and in an electric field, was then devised. The observed deflection in the magnetic field gives the value $\frac{m\,u}{e}$, while the deflection in an electric field gives the value $\frac{m\,u^2}{e}$. Combining these two equations, the values of $\frac{e}{m}$ and u are obtained.

In these, as in the previous experiments, the value $\frac{e}{m}$ was found to be a constant for different gases under different conditions and equal to about 8×10^6—a somewhat lower value than that deduced in the first experiment. Taking into account the difficulty of the experiments, the agreement

between the results of the various methods was as close as could be expected. Since the value $\frac{e}{m}$ for the hydrogen atom is about 10^4, these results indicated that either the mass of the cathode particle was much smaller than that of the hydrogen atom, or that the charge on the cathode particle was much greater. Thomson at first considered that the cathode particle might carry a larger charge than the hydrogen atom, but concluded that the evidence indicated strongly that the mass of the cathode particle was very small compared with that of the hydrogen atom.

The essential points brought out in these experiments were that the cathode particle moved at an enormous speed and had a mass much smaller than the chemical atom, and that the cathode particles were of the same mass whatever the gas through which the discharge passed or the nature of the electrodes.

It is of interest to give at this point some of the conclusions reached by Thomson at this time (1897):

The explanation which seems to me to account in the most simple and straightforward manner for the facts is founded on a view of the constitution of the chemical elements which has been favourably entertained by many chemists: this view is that the atoms of the different chemical elements are different aggregations of atoms of the same kind. In the form in which this hypothesis was enunciated by Prout, the atoms of the different elements were hydrogen atoms; in this precise form the hypothesis is not tenable, but if we substitute for hydrogen some unknown primordial substance x, there is nothing known which is inconsistent with this hypothesis, which is one that has been recently supported by Sir Norman Lockyer for reasons derived from the study of the stellar spectra.

If, in the very intense field in the neighbourhood of the cathode, the molecules of the gas are dissociated and are split up, not into the ordinary chemical atoms, but into these primordial atoms, which we shall call for brevity corpuscles; and if these corpuscles are

charged with electricity and projected from the cathode by the electric field, they would behave exactly like the cathode rays. They would evidently give a value of $\frac{m}{e}$ which is independent of the nature of the gas and its pressure, for the carriers are the same whatever the gas may be: . . .

And again:

Thus in this view we have in the cathode rays matter in a new state, a state in which the subdivision of matter is carried very much further than in the ordinary gaseous state: a state in which all matter—that is, matter derived from different sources, such as hydrogen, oxygen, &c.—is of one and the same kind; this matter being the substance from which all the chemical elements are built up.

Thomson pointed out the possibility that the mass of the corpuscle might be electrical in origin. He had shown in 1881 that the magnetic field set up when an electrified particle is in motion increases the apparent mass of the particle. No definite conclusion, however, as to the origin of the mass of the corpuscle was possible until the experiments of Kaufmann in 1902 on the β rays from radium. He showed experimentally that the mass of these negatively charged particles depended upon their velocity of projection, and deduced from his measurements that the mass of the 'corpuscle' or electron was entirely electromagnetic in origin, and that there was consequently no necessity to assume the presence of a material nucleus on which the charge was distributed.

The paper which we have here considered in some detail is of the greatest importance, for it contains the first definite proof that the cathode rays consist of a stream of negatively charged particles moving at a very high velocity, and possessing a mass very small compared with that of the lightest chemical atom. It does not

fall within the scope of this chapter to consider Thomson's subsequent investigations, in which he showed that corpuscles of the same small mass could be liberated from matter by a variety of agencies.

The importance of Thomson's paper on the cathode rays was immediately recognised, and a number of observers on the Continent repeated the experiments and made determinations of the value of $\frac{e}{m}$ for the corpuscles. We may refer in particular to the fine experiments of Wiechert, who measured directly the velocity of the cathode particles, and to the investigations of Lenard and Kaufmann. The direct measurement by Wiechert[1] of the speed of the cathode particles, and the proof that it agreed closely with the value deduced indirectly as in the methods of Thomson, was of great importance, for it gave the necessary confidence in the correctness of the general electromagnetic theory of a moving charge on which the deductions depended.

While Thomson was investigating the nature of the cathode rays, J. McClelland (1897), who came from Ireland in 1896 to work in the Laboratory, made an examination of the properties of the Lenard rays, i.e. of the rays which appear on the opposite side of a thin metal foil on which the cathode rays impinge. He showed that the Lenard rays, like the cathode rays, carried a negative charge of electricity, and were identical in general properties with the cathode rays. Experiments were made to test whether the cathode rays actually passed through thin metal foil, or whether the foil acted as a secondary cathode during the discharge giving rise to cathode rays on the opposite side. There appeared to be evidence that in some cases the latter was the true explanation. Measurements were

[1] *Wied Ann.*, 69, p. 739 (1899).

also made of the amount of the discharge in a vacuum tube transported by the cathode particles, for it had generally been supposed that the cathode rays played little part in the transference of the current. McClelland showed that at low pressures at least one quarter of the current was carried by the cathode stream.

The nature of the x-rays.—In announcing the discovery of the new type of radiation, now known by his name, Röntgen called them the x-rays, since the nature of the radiation was a matter of conjecture. The x-rays had properties so different from any type of radiation known at that time that no adequate basis of comparison was possible. In addition to their extraordinary power of penetration through all kinds of matter, the rays showed no evidence of direct reflection, refraction, or polarisation, while they differed from the cathode rays not only in their greater power of penetration, but in the absence of deflection by a magnetic or electric field. It was at first thought possible that the rays might prove to be a type of normal or longitudinal wave propagated through the ether. Thomson (1896) took up this question and examined mathematically the conditions under which such longitudinal waves could be propagated. He showed that waves of this type could arise from a medium filled with charged particles in motion such as occur in the discharge through a vacuum tube, and distinguished between two types of longitudinal waves which might be set up. According to Maxwell's theory, such waves could only be propagated if the ether were set in motion and in a space containing a distribution of charged particles. There was no definite evidence, however, to show that the Röntgen radiation could be ascribed to a disturbance propagated in this manner.

Some time later Stokes suggested an interesting theory in explanation of the origin and character of the x-rays.

An electrified particle in motion is surrounded by a magnetic field. When the particle is stopped suddenly it must give rise to an electromagnetic impulse which is propagated outwards as a spherical wave with the velocity of light. If the cathode stream consists of a large number of negatively electrified particles moving at a high speed, the sudden stoppage of these particles must give rise to a great number of pulses following each other in rapid but irregular succession. On this view the cathode rays are the parents of the x-rays, and the latter are to be regarded as a type of non-periodic transverse wave motion in the ether.

The character of the radiation to be expected on this theory was carefully examined by Thomson (1898) in a mathematical investigation published a short time later. He showed that the sudden stoppage of an electrified particle in rapid motion gives rise to a thin electromagnetic pulse in which the magnetic and electric forces in the wave front are exceedingly intense. Under ordinary conditions, when the velocity of the particle is small compared with the velocity of light, the pulse is spherical, but if the particle has nearly the velocity of light, a second pulse is generated which is plane and is propagated only in the original direction of motion of the particle. The pulse theory was shown to account in a general way for the properties of the x-rays. The absence of the characteristic properties of light waves was to be expected if the thickness of the pulse was small compared with the diameter of the molecule. The more sudden the stoppage of the charged particle, the thinner and more penetrating would be the pulse. 'Hard' or penetrating x-rays consisted of thin pulses, 'soft' rays of broad pulses. At the same time, the existence of intense magnetic and electric forces in the pulse offered a reasonable explanation of the ionising property of the radiation.

In a later paper (1898) he applied the pulse theory to

account for the secondary radiation which is always set up when the x-rays fall upon matter. A pulse in passing over an atom sets the component charged corpuscles in motion, and their movement gives rise to an electromagnetic disturbance similar in general character to the primary pulse. This theory has guided a large amount of experimental work done later in the Laboratory on secondary x-rays.

The character of this secondary radiation was at this time under experimental investigation by P. Langevin, who was the first foreign representative to enter the Laboratory under the new regulations. The results of his researches on the secondary rays were not published at the time, but were included in his thesis for the Doctor of Science in Paris, published in 1902 and dedicated to Professor Thomson. This contained an admirable account of the properties of ionised gases.

Conductivity produced in gases by x-rays.—It is difficult to realise to-day the extraordinary interest excited in the lay and scientific mind alike by the discovery of the penetrating x-rays by Röntgen in November 1895. Almost every physical laboratory in the world proceeded to repeat the experiments of Röntgen and to make further investigation of their properties.

Immediately after the announcement of Röntgen's discovery, an x-ray tube was prepared in the Laboratory. Thomson's interest was at once centred on the question whether the new radiation possessed the property of discharging an electrified body. Using an electrometer, he was able at once to show that the rays caused a rapid discharge of both positive and negative electricity, and to about an equal degree (1896). In this respect the x-rays had very different properties from ultra-violet light, which only discharges a negatively electrified body.

This discovery was of the first importance, and immediately opened up a wide field of investigation, for the discharging property served as a delicate quantitative method of measuring the effects of the rays. In the initial experiments (1896), Thomson was assisted by McClelland. It was found that a gas acted on by the radiation became a temporary conductor of electricity, and behaved in that respect like a weak electrolyte. It was natural to ask at once whether solid dielectrics were also made conducting by the rays. Experiments which were made on the rate of discharge of electrified bodies imbedded in insulators appeared to indicate that this was the case. It is now known that many liquid dielectrics become slightly conducting under the influence of *x*-rays or radium rays, but an experimental proof of the conductivity of solid insulators is beset with great experimental difficulties.

The effect of conditions on the rate of discharge through a gas was examined. It was found to depend upon the pressure and nature of the gas. The rate of discharge at first increased with the voltage, but soon reached a maximum value which was not altered by a large increase of the voltage.

In the meantime the conclusion that a gas acted upon by *x*-rays behaved like a weak electrolyte was verified in another way by J. Erskine Murray (1898), who was working in the Laboratory upon the effect of surface and other conditions on the contact differences of potential between metals. He showed that if the *x*-rays fell on two insulated metal plates, initially at zero potential, the plates rapidly acquired the true contact difference of potential. In fact, the gas between the plates when acted on by the rays behaved in this respect exactly as if a drop of weak electrolyte had been placed between them.

At the beginning of the Easter Term of 1896 Thomson

was joined by Rutherford, who had just completed his experiments upon the magnetic detector of electrical waves. A systematic attack was made to discover the nature of the process taking place in the gas which caused it to act as a conductor of electricity under the influence of the rays. There were several suggestive observations that threw a good deal of light on the subject. A gas retained appreciable conductivity a second or more after the radiation was cut off. This effect was most simply examined by passing a rapid current of air along a metal tube, at one point of which the rays passed through the walls and made the gas conducting. It was found that the gas retained its discharging property after being carried several feet from the point of action of the rays. If, however, the conducting gas passed through a strong electric field, its power of discharging was completely lost. At the same time it was observed that the discharging property of the gas disappeared by passing it through a plug of glass or cotton wool, or by bubbling it through water.

It was thus clear that the rays produced some type of structure in the gas, which was either destroyed or removed by an electric field or by passing it through finely divided substances. The view that the rays 'ionised' the gas, i.e. produced a number of positively and negatively charged particles throughout the volume of the gas, was soon seen to offer a satisfactory explanation of the experimental facts. The after-conductivity of the gas was to be ascribed to the slowness of the recombination of the ions produced in the gas while the rays were acting. The removal of the conductivity by a strong electric field was due to the rapid motion of the positive ions to the negative electrode and *vice versa*. This theory was seen to give a simple and rational explanation of the fact that the conducting gas did not obey Ohm's law. When a strong electric field is applied,

the ions are swept so quickly to the electrodes that very few have time to come together and recombine. Under these conditions the current through the gas is a maximum, and provided the electric field does not of itself produce new ions, the current should then be independent of the voltage applied. The existence of this maximum or 'saturation' current is a characteristic property of all ionised gas, provided the voltage applied is well below the value required to produce spark discharge.

The simple mathematical theory of the conduction of electricity through gases, which was published in the *Philosophical Magazine* in November 1896, has formed the basis of all subsequent work on this subject, and has been found to account in a satisfactory manner, not only for gases made conducting by *x*-rays, but also by a variety of other ionising agencies. In this theory the radiation was supposed to produce a kind of dissociation of a minute fraction of the neutral molecules, resulting in the appearance of a number of positively and negatively electrified carriers or ions throughout the volume of the gas. No assumption was made as to the nature of the ions themselves.

There still remained a large amount of work to verify experimentally the main assumptions on which the theory was based. It was assumed as probable by Thomson that the rate of recombination of the ions was proportional to the square of the number present. This was verified by Rutherford (1897), who determined experimentally the relative number of ions present in the gas at different intervals after the rays were cut off. The rate of recombination was found to be much affected by the presence of dust or other nuclei in the gas. As the ions diffused through the gas they came in contact with the relatively large dust particles and adhered to them or gave up their

charge. In this way the dust particles acted as intermediaries for promoting the rapid disappearance of the ions.

It was also important to know the velocity with which the ions travelled in a known electric field. Since in a gas at ordinary pressure the ion moves in a retarding medium, it should rapidly acquire a maximum or terminal velocity in a constant electric field and then move forward with constant velocity which should be proportional to the strength of the electric field. It was convenient to express the mobility of the ion in terms of the velocity acquired when it moved under a potential gradient of one volt per centimetre. By a special method, Rutherford (1897) determined the sum of the mobilities of the positive and negative ions produced in a number of gases at atmospheric pressure and temperature. In air the sum of the mobilities was found to be 3·2 cm. per second and in hydrogen 10·4 cm.

From considerations based on the kinetic theory of gases it was deduced that a molecule of hydrogen, if it carried a charge equal to that of the hydrogen atom, should travel in hydrogen at the rate of 340 cm. per second for a potential gradient of one volt per centimetre, while the observed sum of the mobilities of the two ions was less than one-thirtieth of this. On the assumption that each ion carried the same charge as the hydrogen atom in electrolysis, this indicated that the mass of the ion under ordinary conditions was much greater than that of the molecule of the gas in which it was produced. This suggested that the charged ion immediately after its formation became the centre of an aggregation of neutral molecules which travelled with it. This idea did not seem improbable, and gained still further support from the later observation that the velocity of the negative ion is not a constant, but is much affected by the presence of water vapour in the gas.

There is now considerable evidence that the large apparent mass of the ion is to be ascribed, not entirely to the addition of a cluster of neutral molecules, but rather to the effect of the charge, which causes it to make far more collisions with the gas molecules in traversing a given distance than if it were uncharged. The ion consequently experiences a greater resistance to its motion than the uncharged molecule, and this gives it the appearance of having a much greater mass.

In the course of their experiments to prove the existence of ions in gases during the passage of an electric discharge, both Schuster and J. J. Thomson had noted some results which indicated that the negative ion was more mobile than the positive. In the initial experiments with *x*-rays no certain difference had been observed in the properties of the two ions apart from the sign of their charge. Zeleny (1898), however, showed that there was a distinct difference between them, the negative ion always moving faster than the positive in a given electric field.

The difference in the mobilities of the two ions was first noted in experiments where a rapid current of air was directed close to the surface of one of two parallel charged plates between which the gas was ionised by the *x*-rays. When the air was blown past the negative plate, far more positive electrification was observed in the issuing gas than negative electrification when the potential was reversed. This indicated that the negative ion moved faster than the positive. This conclusion was confirmed by a number of experiments. A method was devised for measuring separately the velocity of the positive and of the negative ion. In most gases the negative ion moved from 15 to 25 per cent. faster than the positive. In later experiments it was shown that the velocity of the negative ion was much influenced by the presence of moisture in the gas, and that the greatest

difference between the velocities of the two ions is found in dry gases.

The excess of ions of one sign near the electrode when an electric field acts on a uniformly ionised gas should disturb the potential gradient existing before the gas was acted on by the rays. This was experimentally observed by Zeleny, who showed that there is a rapid drop of potential near each electrode. On account of the greater mobility of the negative ion, the drop of potential is greater near the positive electrode.

This disturbance of the potential gradient due to the movement of the ions was independently observed by Child when working in Berlin. He came to the Cavendish Laboratory, and continued his experiments.

Zeleny (1898) showed that the movement of the ions in an electric field set up convection currents in the gas. A number of simple experiments were devised to illustrate this effect.

The ionisation produced by a given intensity of *x*-rays varies greatly with the nature of the gas through which the rays pass. It is especially large in gases of the halogen group and in heavy vapours like those of mercury and methyl iodide. Rutherford (1897) showed there was a close connection between absorption of the radiation and the amount of ionisation it produced. The results indicated that about the same amount of energy was required to produce a pair of ions, whatever the gas or vapour.

J. J. Thomson (1898) made a number of experiments to determine the relative conductivity of different gases and to test whether the ionisation of gases was an additive property. For example, it was important to settle whether the conductivity of a complex gas like NO could be deduced from a knowledge of the conductivity of each of the component gases. It was shown that the additive law held

approximately for a number of gases. Such a relation indicated that ionisation consisted in a process taking place in the atom itself, and not in the separation of the atoms in the complex molecule.

Discharge of electricity by ultra-violet light.—As soon as the ionisation theory had been shown to account in a satisfactory way for the conductivity produced in a gas by x-rays, attention was directed to other types of discharge to see whether a similar explanation was valid. The remarkable property of ultra-violet light of causing the rapid discharge of a negatively electrified body was first observed by Hertz and investigated in great detail by Elster and Geitel and others. The experiments of Lenard and others had indicated that the loss of electricity observed might be due to a disintegration of the metal surface by the action of the ultra-violet light. It was important to settle whether this was the true explanation, or whether the discharge was due to the ionisation of the gas close to the metal surface. Zeleny (1898) and Rutherford (1898) independently showed by blowing a rapid stream of air close to the surface of the charged plate on which the light fell that the loss of charge was due to a convection of negatively charged carriers.

Rutherford made a number of experiments to determine the velocity of these carriers. He found that the velocity of the carrier was the same for all metals, and within the limit of error agreed with the velocity of the negative ion produced in the same gas by x-rays. It was thus clear that the negative ion produced by ultra-violet light was identical in mass with the ion produced by x-rays.

From the later experiments of Thomson, Lenard, and others, it is now known that the ultra-violet light sets free negative corpuscles at the surface of the metal. If the pressure of the surrounding gas is not too low, the

corpuscle attaches itself to a molecule or group of molecules, and becomes the negative ion. At very low pressures the corpuscle remains free, and is identical in mass with the cathode ray particle. The velocity of the negative ion was found to vary inversely as the pressure of the gas over a wide range.

Uranium radiation.—Shortly after the discovery of the radioactivity of uranium, Becquerel showed that the radiation from uranium, like the x-rays, possessed the property of discharging electrified bodies. After completing the experiments on the discharge due to ultra-violet light, Rutherford (1899) made a systematic examination of the conductivity produced by uranium rays. He showed that the radiation ionised the gas throughout its volume, and that the ions were identical in character with the ions produced under similar conditions by x-rays. The effect of various conditions on the rate of discharge was closely examined.

In the course of this investigation he showed that uranium and its compounds gave out two distinct types of radiation, one, very easily absorbed, called the α rays, and the other, far more penetrating, called the β rays. The absorption properties of these two types of radiation were investigated. This investigation, which was published after Rutherford had left the Laboratory to take up the position of Professor of Physics in McGill University, Montreal, laid the foundation of the electrical method of comparison of the radiations from radioactive bodies. In this work the irregularity of the radiation from thorium compounds was first noted. This observation ultimately led to the discovery of the thorium emanation.

Conductivity of flame gases.—It had long been known that flames and flame gases possessed the property of discharging electrified bodies. McClelland (1898) made a number of experiments to test whether this conductivity

could be ascribed to the presence of charged carriers or ions. In order to avoid the complicated effects produced by the introduction of electrodes into the flame itself, experiments were made on the conductivity of the flame gases at some distance from the flame. The current through the gas was found to increase at first with the voltage, but soon reached a 'saturation' value, as in the case of gases ionised by x-rays. The conductivity fell off rapidly with the distance from the flame, indicating that the positive and negative carriers rapidly recombined. Measurements were made of the velocity of the positive and negative carriers. As in the case of ions produced by x-rays, the negative ion had a velocity about fifteen per cent. greater than that of the positive. The velocity of the ions themselves was not constant, but decreased rapidly with decreasing temperature of the flame gases, and was much smaller than the velocity of the ions due to x-rays. These results indicated that the ions after production increased in mass due to the condensation of matter upon them, and that the mass of the ion increased as the temperature fell.

The experiments of McClelland showed that the ionisation theory gave a satisfactory explanation of the conductivity of flame gases. The application of the same principles to explain the complicated phenomena in the flame itself was made later by H. A. Wilson. Much of our knowledge of this important subject is due to his researches, which, however, hardly fall within the period now under consideration.

Properties of electrified gases.—It is an interesting coincidence that, while the experiments were in progress in 1896 to explain the conductivity of gases exposed to x-rays, another distinct but allied field of investigation was opened up by Townsend. Townsend and Rutherford came to Cambridge the same day—the one from Ireland, and the other from New Zealand—and were the first 'advanced

students' to enter the Laboratory and the first to receive the B.A. degree or the Certificate of Research.

After completing his first investigation on the magnetisation of liquids, Townsend turned his attention to the important question whether chemical actions were accompanied by electrical effects. It was natural to begin experiments upon newly prepared gases, as it is an easy matter to test whether a gas is electrified or conducting.

It is of interest to note that Laplace and Lavoisier were aware that hydrogen, evolved when a metal dissolves in acid, carries an electric charge. Enright, in 1890, had made a number of investigations on this subject. After repeating some of the experiments of the latter, Townsend (1897 and 1898) tried whether the gases liberated in electrolysis were electrified. He found that under certain conditions these gases were very highly charged. For example, in the electrolysis of dilute sulphuric acid both the hydrogen and oxygen evolved have a positive charge, while in the electrolysis of caustic potash both carry a negative charge.

These charged gases had most remarkable properties. They could be passed through finely divided matter like cotton wool and bubbled through liquids without losing their charge, while the gas when stored in a vessel retained some of its charge for a considerable time. On bubbling the charged gas through water a dense white cloud appeared. This vanished when the gas was bubbled through a drying agent, but reappeared again when the gas passed into a moist atmosphere. Townsend concluded that the charge was not carried by the gas itself, but was due to the presence of a number of charged particles. These carriers diffused very slowly and moved very slowly in an electric field, showing that they must have a mass much greater than the ordinary ion in gases.

These electrified gases behaved very differently from the

electrified gases obtained by blowing out the positive or negative ions produced by x-rays. The latter give up their charge completely in passing through a plug of cotton wool or bubbling through water, and are removed from the gas very rapidly by the electric field or by processes of diffusion.

The origin of the charged carriers in the experiments of Townsend raises questions of some difficulty. In later experiments to investigate this point, H. A. Wilson (1898) found he could produce similar charged gases which formed a cloud by aspirating air rapidly through some solutions. It seemed possible that the nucleus of the charged drop in these cases was due to a small quantity of solid matter left behind by the evaporation from a small drop of the solution.

In the course of experiments on the time required for the loss of charge of electrified gases, the attention of Townsend was directed to the importance of diffusion phenomena in these cases. In an important paper in the *Philosophical Magazine* (1898), he mathematically investigated the following problem: Given two gases, A and B, contained inside a vessel, the walls of which absorb A, what quantity of A will remain unabsorbed after a given time has elapsed? The solution of the problem applies not only to electrified or ionised gases, but gases in general. In the case of ionised gas the problem is much simplified by supposing that the number of ions present is very small compared with the number of gas molecules. He showed that the rate of diffusion of ions was intimately connected with their velocity in an electric field. For example, the negative ion, which moves faster than the positive ion in an electric field, also diffuses faster. As a consequence of this there is always an excess of positive electricity in an ionised gas which has passed along a tube. The loss of conductivity of a gas due to diffusion in passing through tubes was shown to increase rapidly with decrease of diameter of the tube. This explains

why the ions lose their charge in passing through a plug of cotton-wool.

In this paper we have the foundation of the theory which was afterwards applied in his experiments to determine the rate of diffusion of the ions, and his proof that the ion carried the same charge as the hydrogen atom.

Condensation nuclei.—We shall now consider a remarkable series of investigations by C. T. R. Wilson on the conditions for the condensation of water in dust-free gases saturated with water vapour. These researches have not only added greatly to our knowledge of the problem under investigation, but have opened up a novel and striking method of investigating the properties of ionised gases.

It had long been known that a small expansion is sufficient to cause the appearance of a dense fog in ordinary air saturated with moisture. A sudden expansion of the air causes a lowering of its temperature and consequent supersaturation. The excess of water vapour then condenses upon the dust nuclei, always present in ordinary air, and causes a dense fog composed of minute drops of water. If the dust nuclei are initially removed from the air by filtering or by successive condensations, a small expansion of the air no longer produces a cloud. The behaviour of dust-free air saturated with water vapour and allowed to expand suddenly had been previously investigated by a number of observers, notably by Aitken and R. v. Helmholtz, but no certain evidence had been obtained that condensation could occur in the gas even when the expansion was sufficiently great to produce a considerable supersaturation. C. T. R. Wilson found that a gas could be completely deprived of nuclei by successive small expansions, allowing time for the drops to settle between the expansions. A special apparatus was constructed to make the expansion very sudden, so that the lowering of the temperature, and consequently the

degree of supersaturation, could be calculated from the change of volume without any appreciable correction for the gain of heat from the walls of the vessel during the time of the expansion. After the dust nuclei had been removed, no condensation was visible until the ratio of the final to the initial volume of the air was 1·252, when a small number of drops like fine rain appeared in the expansion chamber. With increasing amount of expansion the number of these raindrops remained practically unaltered until an expansion of 1·375, when a dense fog of fine drops appeared. The number of nuclei rapidly grew with further increase of the expansion, while the drops became finer and settled more slowly.

It was clear from these experiments that two types of nuclei capable of condensation of water vapour appeared in dust-free air. Nuclei of the first type were few in number and required about a fourfold supersaturation to be effective; those of the second type were very numerous and required about an eightfold supersaturation. The amount of expansion required for condensation on these nuclei was sharply defined.

A preliminary account of the initial experiments was communicated by Wilson to the *Proceedings of the Cambridge Philosophical Society* in 1895. The final paper appeared in the *Transactions of the Royal Society* in 1897.

The experiments with the initial apparatus had already been completed at the time of the discovery of the *x*-rays.

An *x*-ray bulb was obtained and experiments were made at once to try whether the conditions of condensation were altered by allowing the rays to pass through the air in the expansion chamber. No nuclei were observed until the expansion 1·258, required for rain-like condensation in ordinary air, was reached. At this stage, instead of the

few drops ordinarily observed, a dense fog appeared which took more than a minute to settle.

This fog did not appear if several seconds elapsed between cutting off the rays and applying the expansion. This showed that the nuclei produced by x-rays rapidly disappeared from the air after the rays were cut off. A preliminary account of this striking experiment was communicated to the Royal Society on March 3, 1896. Later Wilson (1897) observed that the rays given out by uranium had an effect similar to that of the x-rays.

These experiments gave rise to the obvious suggestion that the two kinds of nuclei which cause condensation are the two kinds of ions, positive and negative, which had been shown to exist in ionised gases. But the description of the method by which the identification of the condensation nuclei with the ions was established, and of the important researches to which that identification led must be left to the succeeding chapter.

Theory of moving charges.—We have seen that Thomson in 1881 had proved that a moving charged body behaves as if it had an additional electrical mass in virtue of its motion. The mathematical theory of this subject had been developed and extended later by Heaviside. Searle, following Heaviside's methods, developed the general theory of such moving charges in an important paper communicated to the *Transactions* of the Royal Society in 1896. In 1897 he communicated to the *Philosophical Magazine* an investigation of the theory of a charged ellipsoid in motion. The great importance of such investigations became manifest in later work on the constitution of the negative corpuscle or electron, where the theory of moving charges was employed to prove that the mass of the corpuscle is entirely electromagnetic in origin.

Passage of electricity through gases and liquids.—In

1895, Capstick, after completing an investigation of the ratio of the specific heats of gases and liquids, determined the cathode fall of potential in a number of gases (1898). Warburg had shown that the fall of potential between the cathode and a point near it was a definite constant for each gas under certain conditions of the discharge. It was of interest to examine the value of the cathode fall in complex gases, and to see whether it was connected with other physical and chemical properties of the gas. In some cases it was found that the cathode fall for complex gases approximately followed an additive law, but the deduction of results was complicated in some cases by the dissociation of the complex molecules by the discharge.

J. Henry (1898) examined the effect of a magnetic field on the electric discharge through rarefied gases at a pressure of about 1 cm. of mercury. The discharge under these conditions is tubular in shape, and is deflected by a magnetic force at right angles to the discharge, in the same way as a flexible wire conveying a current. The effect of the nature of the gas and of other conditions on the amount of deflection was carefully examined.

In 1889 Koller concluded that the conductivity of certain liquids decreased as the thickness of the layer decreased. Since in gases there was some evidence that a discharge could not pass readily if the electrodes were too close, it was important to examine whether a similar effect existed in liquids. This subject was taken up by G. H. Bryan (1898). The experiment was a difficult one, involving the measurement of the conductivity of very thin layers of liquids. After taking every precaution in obtaining clean electrodes and getting rid of polarisation effects, it was found that the conductivity of a liquid in thin layers was the same as in thick layers.

Magnetism.—In 1895, G. F. C. Searle (1896) devised an

ingenious method of determining the hysteresis loss in a specimen of iron by a single observation. An apparatus was constructed on the dynamometer principle, the magnetising current passing through the fixed coil, and the current induced in a secondary coil passing through the movable coil. By this method, on reversal of the magnetising current, the throw of the movable coil is a measure of the hysteresis loss in the specimen. This simple method of measurement is very advantageous in researches upon the effect of conditions on the hysteresis loss in iron.

On entering the Laboratory in 1895, Townsend (1896) made an accurate determination of the magnetic susceptibility, k, of a number of solutions of iron. For this purpose he employed a sensitive balance-induction method. The value of k was found to depend only on the quantity of iron in the solution, provided it was either in the ferric or ferrous state, but varied in a fixed ratio for these two conditions. The value of k was found to decrease rapidly with a rise of temperature of the solution.

While in New Zealand, Rutherford (1897) had investigated the effect of high-frequency discharges on the magnetism of iron. A magnetised needle is partially demagnetised by the passage of electrical oscillations through a solenoid surrounding it. The decrease of magnetism is always greater if the first oscillation of the discharge is in the right direction to reverse the magnetism. This property was used as a quantitative method of finding the damping of the oscillations in the Leyden jar discharge, and of measuring the resistance of conductors for high-frequency currents. These experiments were continued in the Cavendish Laboratory.

By using very fine magnetised steel wires surrounded by a fine solenoid, this demagnetising property proved a

very sensitive quantitative method for detecting electrical waves. Using large Hertzian vibrators, the electrical waves emitted were observed by means of the magnetic detector for a distance of about half a mile. These experiments were made before Marconi began his well-known investigations on signalling by electric waves. This effect of electric oscillations of altering the magnetism of iron is the basis of the magnetic 'detectors' developed by Marconi and others, which have proved one of the most sensitive and reliable of receivers in radiotelegraphy.

General Physics.—W. C. Craig Henderson and J. Henry (1897), at the suggestion of Professor Thomson, tested experimentally whether the ether was set in motion by electrical oscillations. According to theory, when a wave passes through the ether a mechanical force acts on the ether in the direction of motion of the wave. If the waves are undamped, the force is periodic and on the average zero; if damped, this mechanical force should have a finite value, and it was of interest to try whether the ether was set in motion by this force. An interferometer method was adopted, so that a change of velocity of a beam of light could be detected by the shift of interference fringes. The spark of a Leyden jar was used as a source of light, while the discharge produced rapidly damped oscillations round a suitably arranged electric circuit. No appreciable shift of the interference fringes was observed, indicating that, if the ether moved at all, its velocity was very small.

J. H. Vincent published a number of papers on the photography of ripples (1897 and 1898). Waves were set up in a mercury surface by means of tuning-forks, and a large number of beautiful photographs were obtained illustrating many of the properties of wave motion in general. While in the Laboratory he constructed a model (1898) which illustrated experimentally the formula given

by Helmholtz for the change of velocity of propagation of waves in a medium capable of absorption.

Wade devised a sensitive differential method for determining the boiling-points of liquids (1897). In the ordinary direct method it is difficult to obtain the boiling-point accurately, since the correction due to barometric pressure is often considerable. By Wade's method the difference in boiling-point between the solution and pure water is accurately measured by a differential platinum thermometer, and this difference is independent of atmospheric conditions. Observations were made on the boiling-points of a number of solutions of different concentration and the results compared with theory.

W. C. D. Whetham (1897) in making experiments upon the connection between the dielectric constant of a liquid and the conductivity produced when this liquid is used as a solvent, observed that a solution of water in formic acid had a high conductivity. At his suggestion, V. Novak (1897), a foreign research student, made an accurate and systematic examination of the depression of the freezing-point and of the conductivity of solutions of water in formic acid. He found that over a certain range of concentration the increase of conductivity and the depression of the freezing-point were proportional to the amount of water added. These results indicated that the water was not ionised by the formic acid.

Subsequent to the discovery of *x*-rays, it was observed by many experimenters that metals affected a photographic plate. This was first thought to be due to a type of radiation emitted by the metals. This effect is very marked in the case of zinc. It was important to settle whether the darkening of the photographic plate was due to an actual radiation or some indirect chemical action. By blowing a current of air between a zinc surface and a photographic

plate, Thomson (1897) found that the image obtained on the plate was displaced and distorted in the direction of the air current. This showed that the effect must be ascribed to an action of a vapour from the metal on the plate.

CHAPTER VII

(1899–1902)

The first direct determination of the charge carried by the ions in conducting gases, completed by Thomson towards the end of 1898, forms a fitting starting-point for a new chapter.

Townsend's experiments (1898) described in the last chapter were the first to give a direct measurement of what could, with a high degree of probability, be regarded as being the elementary quantity of electricity, and the value found for this charge—about 5×10^{-10} electrostatic units—is in good agreement with the most recent determinations. The charged particles studied by him were not, however, free ions such as are produced in gases by Röntgen rays, but much larger bodies, and their charge might be regarded as in a sense accidental. For it is possible to obtain particles, possessing the same property of condensing moisture upon themselves to form a visible cloud even in unsaturated air, but carrying no charge of electricity. It was indeed conclusively proved by H. A. Wilson (1898) that this property is not in any way due to the charge carried by the particles. Townsend (1899) himself also made a study of uncharged nuclei having this power, in an investigation which did much to clear up a subject about which there had been much controversy—the nature of the clouds formed when ozone is passed through certain solutions.

Thomson's method of obtaining the ionic charge depended upon the possibility of making the ions visible by condensing water upon them. C. T. R. Wilson's earlier work of 1896–7 had proved that in gases exposed to x-rays or uranium rays condensation nuclei of a very definite kind are produced, a few such nuclei being also present in dust-free air under normal conditions. The experiments did not definitely prove that the nuclei in question are identical with the ions; all that was claimed for them is contained in a statement in a paper read before the Cambridge Philosophical Society in October 1897:—

> The electrical properties of gases under the action of Röntgen and uranium rays point to the presence of free ions. It is natural to identify with these the nuclei made manifest in the gas under the same conditions by the condensation phenomena described in this and previous papers.

The remarkable effects which an electric discharge has upon the appearance of a steam jet had been studied by several observers and they had been attributed by R. v. Helmholtz as early as 1887 to the presence of free charged atoms or ions; the matter was further developed both on the theoretical and experimental side by Thomson in 1893, in a paper on the effect of electrification and chemical action on a steam jet.

When therefore the condensation of water vapour in dust-free gases was studied by means of the expansion apparatus and the existence of the two definite classes of nuclei referred to in the last chapter was discovered, it was not unnatural to think that the nuclei giving rise to the rainlike condensation might possibly be free ions; for their number was always small and varied in different gases, and moreover the degree of supersaturation required to cause condensation upon them was consistent with the view that they were ions, according to the estimates that had been

made of the magnitude of the atomic charge. It was indeed this idea that suggested to Wilson the experiments in which the production of condensation nuclei by x-rays was first observed in March 1896. Thomson had just shown that gases become conductors of electricity under the action of the rays. It was thought that if the rainlike condensation were due to free ions, then exposure to x-rays might be expected to leave unaltered the expansion required to cause condensation, while increasing greatly the number of drops produced if this critical expansion was exceeded. A number of x-ray bulbs having been made in the laboratory by Everett, Professor Thomson's assistant, for other investigations, the experiment was very easily tried, and the x-rays were found to produce large numbers of nuclei identical, as regards the degree of supersaturation required to form drops upon them, with those which gave rise to the rainlike condensation. The obvious interpretation of these experimental results was that we had probably here a method of making the individual ions visible, and the possibility of its ultimate application to the determination of the ionic charge was at once plain. But the direct proof was wanting that these nuclei are the ions, whose existence had been postulated to explain the phenomena of the conduction of electricity through gases exposed to x-rays or other radiations.

That the nuclei produced by x-rays are charged bodies, moving in an electrical field with velocities of the same order as had been deduced for the ions by Rutherford, was proved independently by J. J. Thomson (1898) and C. T. R. Wilson (1899) in the course of investigations completed during 1898.[1] Thomson began his attack on the problem of the determination of the charge carried by the ions

[1] Wilson's paper, though published in 1899, was read before the Royal Society in November 1898.

produced by x-rays by proving that the nuclei produced by the rays are removed by an electric field.

Two parallel plates were placed in the vessel containing the dust-free air; these plates were about 5 centim. apart and were large enough to include the greater part of the air between them. The plates could be connected with the terminals of a battery of small storage cells giving a potential difference of about 400 volts. Röntgen rays passed through the gas between the plates; this had previously been freed from dust. When the plates were disconnected from the battery expansion produced a dense cloud; when, however, the plates were connected with the battery only a very light cloud was produced by the expansion, and this cloud was almost as dense when the Röntgen rays did not pass through the air as when they did.

Wilson, continuing his work on condensation nuclei, found that the various methods which give ions identical with those produced by x-rays (as shown by the work of Rutherford and others on their velocities in an electric field) give also nuclei all requiring the same degree of supersaturation to catch them. It was found for example that a negatively charged zinc plate, exposed to ultra-violet light, gives such nuclei while a positively charged plate does not, and that under suitable conditions the nuclei produced by the point discharge are of the same nature. But on the other hand it was also found that weak ultra-violet light acting on moist air gave nuclei requiring practically the same degree of supersaturation to cause drops to form upon them, whereas no conducting power was known to be produced by that means. An apparatus was therefore constructed to compare under identical conditions the effect of an electric field upon the nuclei produced by ultra-violet light and upon those due to the action of x-rays. It was found that while the x-ray nuclei were completely removed by an electric field such as would have removed the ions, even strong fields

produced no visible diminution in the number of the ultra-violet light nuclei. The most conclusive form of the experiment was that in which the rays were cut off before the expansion took place. With *x*-rays an expansion made three seconds after the rays were cut off gave a fog in the absence of any difference of potential, whereas, when the potential difference amounted to 240 volts, no drops at all were produced even when the expansion was brought about within two seconds after cutting off the rays, or indeed when it was effected as quickly as possible after the rays were stopped. All ions of the kind for which Rutherford measured the velocity would have been removed by the field used in about 1/150 of a second after the rays were cut off; the nuclei agree in their behaviour with such ions. When the air was exposed to ultra-violet light in place of the Röntgen rays, no diminution of the number of the nuclei by the action of the electric field could be detected even three seconds after the radiation was cut off; if these nuclei travel at all under the influence of the electric field they move at least 300 times as slowly as the ions produced by *x*-rays.

The *x*-ray nuclei were thus shown to be the ions whose properties had already been studied by the electrical method, while those produced throughout the volume of a mass of moist air traversed by ultra-violet light are entirely different bodies, although when the intensity of the light is feeble they resemble the ions in the degree of supersaturation required to make water drops form upon them. In air exposed to ultra-violet light which is not very weak the nuclei were found to grow in size, becoming finally large enough to be caught with the smallest possible expansions; if the radiation be sufficiently intense they ultimately become large enough to be visible under suitable illumination even in unsaturated air. Uncharged nuclei,

which might, like the ultra-violet light nuclei, easily be mistaken for ions were found to be produced by the action of moist air on certain metals—on those namely which were found by Russell to affect a photographic plate in the dark.

Thomson's apparatus for determining the charge carried by the ions consisted essentially of an expansion apparatus, of which the cloud chamber was bounded above and below by conducting surfaces, between which a small potential difference was maintained, while the air in the apparatus was exposed to a constant source of x-rays. In any such determination two quantities have to be measured, a charge of electricity and the number of ions which carry this charge. In Thomson's experiments the quantity of electricity involved is the sum of the charges on the free ions of both signs in each c.c. of the gas; this was determined by measuring the current traversing the gas for the known small potential difference between the electrodes, and making use of Rutherford's value of the mean velocity of the ions under unit potential gradient. For the current per square cm. is $nueX$, where n is the total number of ions per c.c., u is the mean velocity of the positive and negative ions under unit potential gradient, e is the charge carried by each ion, and X is the potential gradient. The number of ions was obtained by causing a sudden expansion of the moist gas sufficient to cause water to condense upon them. It was not found convenient to use a direct method of counting the drops; the number was deduced from a knowledge of the size of the drops and of the total quantity of condensed water in each c.c. of the cloud, as in Townsend's experiments. The method used for determining the size of the drops was also that which had been employed by Townsend—the rate of fall of the upper surface of the cloud being measured and Stokes' formula for the motion of a sphere in a viscous fluid

applied. The total quantity of water composing the drops could not, however, be found by weighing the cloud, as Townsend had done; an indirect method was employed, the amount of water separating out from each c.c. of the air as a result of a known expansion being calculated.

The value found for the charge carried by each ion was 6.5×10^{-10} electrostatic unit.

Before referring to later determinations of the same quantity, some further experiments on the behaviour of ions as condensation nuclei must be considered.

In all C. T. R. Wilson's condensation experiments on ionised air, with the exception of those in which the ions were escaping from a negatively charged zinc plate under the influence of ultra-violet light, both positive and negative ions were present; the measurements made were of the supersaturation required to cause condensation on the most efficient nuclei. Now when successively greater and greater expansions, exceeding the least required to form drops in ionised air, are made, the number of drops produced increases very rapidly at first, and a stage is soon reached (if the apparatus gives a sufficiently sudden expansion, and the ionising radiation is not too intense) beyond which there is comparatively little change in the appearance of the resulting shower or cloud until we get the dense cloudlike condensation which is independent of the presence of the ions. Indications had, however, been noticed of an increase in the number of the drops at a point about half way between these two limits. Thomson, in his paper on the determination of the ionic charges (1898) suggested that this intermediate point, which he also noticed, might mark the critical expansion for ions of one sign—the positive, for example—only the ions of contrary sign being caught with smaller expansions; and he pointed out that such a difference in the efficiency of positive and negative ions might

possibly have meteorological applications. For it might under appropriate conditions give rise to the production of an electric field, and he suggested it as a possible source of the normal positive electrification of the atmosphere, an excess of negative electricity being carried down by precipitation. Wilson had already, in the summer of 1898, made some preliminary attempts to study the condensation phenomena in air containing ions of one sign only, the method used being essentially the same as that which was afterwards successful. Thomson's suggestion as to the possible meteorological importance of the question made it possible for Wilson to continue the investigation after being appointed by the Meteorological Council to carry on research with the object of throwing light on the phenomena of atmospheric electricity. It was found that while the negative ion requires a fourfold supersaturation of water vapour to make it grow into a visible drop, something like a sixfold supersaturation is required by the positive ion.

It now appears from the investigations of Przibram, at one time a worker in the Cavendish Laboratory, and from those of Laby (1908) which were carried out there, that water is exceptional in its behaviour. In all the other vapours investigated the positive ion, not the negative, is the one on which the condensation more readily takes place. There is, as Przibram and Laby point out, a similar difference between water and the other substances investigated as regards the sign of the electrification produced by bubbling or splashing—the Lenard effect.

Thomson repeated his determination of the ionic charge in the winter of 1902–3, the method being essentially the same as before but with various improvements. A more constant radiation was used, a radium salt being now the source employed. The introduction of the Dolezalek

electrometer made it possible to measure the electrical quantities more accurately, and in other ways the electrical part of the measurements was improved. But the most important difference was probably in the proportions of the expansion apparatus. In the earlier determination in modifying Wilson's apparatus to make it suitable for the electrical measurements the rapidity of expansion had been too much reduced, and in consequence all the ions had not been captured. The apparatus used in the later experiments was free from this defect, and it appeared from the results obtained with it that the negative ions only had been caught in the earlier experiments, although the expansion should, if rapid enough, have been sufficient to cause water to condense on the positive ions also. The new value found for the charge on the ion amounted to only about half that previously found; the difference was attributed to only half the ions (the negative only) having been counted in the early experiments, twice the true ionic charge being thus assigned to each. Thomson's paper was published in March 1903. H. A. Wilson (1903) had also independently been carrying out measurements of the same quantity, and his results were published in the following month. His method was quite different from Thomson's, though it likewise depended on making the ions visible by condensation of water upon them, and on the measurement of the rate of fall of the drops. By measuring the rate of fall of a cloud under the action of gravity alone and when assisted by a known electric field, the charge carried by each drop, *i.e.* the ionic charge, was directly determined. In this method the charge measured is that carried by each drop, and no electrometer, other than the suspended drops themselves, is required for the measurement. Wilson's results and those of Thomson's later experiments agreed fairly closely. There is now little doubt that they are somewhat too low.

H. A. Wilson found that in many cases drops were present in his clouds which, instead of having the charge of one ion, carried twice or three times or some higher multiple of the ionic charge. Such drops containing multiple charges have been employed in the most recent modification of the cloud method of obtaining the ionic charge—that of Professor Millikan of Chicago.

The extremely valuable work of relating the charge carried by the gaseous ion to the fundamental constant of electrolytic conduction in liquids was accomplished by Townsend in an investigation of which the results were communicated to the Royal Society in April 1899. He was at this time Clerk-Maxwell Student. Townsend showed how, from a comparison of the coefficients of diffusion of ions into gases, and of their velocities under unit potential gradient in the same gases, we could deduce the value of Ne, where N is the number of molecules in a cubic cm. of a gas at standard pressure and temperature, and e is the charge of a gaseous ion. He determined the coefficient of diffusion of the ions in several gases, and with the aid of Rutherford's values of the velocities of the ions he deduced the value of Ne, obtaining approximately identical numbers in each case. Knowing e we can deduce N, the number of molecules in a c.c. of a gas at standard pressure and temperature, and hence the absolute values of the molecular weights—independently of any reference to the facts of electrolysis. The quantity of electricity which has to be passed through an electrolyte to set free one c.c. of hydrogen gives us NE, where E is the charge associated with one atom of hydrogen in electrolysis. Townsend found Ne equal to NE, indicating the equality of e and E, the charges carried by the gaseous ions and by the hydrogen atom in liquid electrolytes respectively.

Townsend extended his diffusion investigations to the

ions produced by other methods, and the results of his experiments on 'The Diffusion of Ions Produced in Air by the Action of a Radio-active Substance, Ultra-violet Light, and Point Discharges' were communicated to the Royal Society in May 1900. As in the earlier experiments, the negative ions were always found to diffuse more rapidly than the positive, in agreement with their greater velocity in a given electric field. The rate of diffusion of the ions produced by the point discharge was found to be smaller than that of the ions set free by the other methods, except in the case of negative ions in moist air.

Townsend's original method of determining *Ne* consisted in finding the ratio between the velocity of the ions in a field of unit electric force and the constant of diffusion—the two constants being found by quite independent investigations. The variations in the value of *Ne* found for different gases and for positive and negative ions were no greater than might have been accounted for by differences of temperature, dryness and purity of the gases in the two investigations. Townsend has quite recently used a method in which separate determinations of the two constants are not involved, and *Ne* is obtained directly—much greater accuracy being in this way rendered possible. The method is specially applicable to the comparison of the charges carried by the positive and negative ions respectively, and Townsend has found that the positive ion may carry a charge which is either equal to or double that of the negative ion according to the nature of the ionising radiation. In the latter case twice as many negative as positive ions are produced by the rays.

Townsend's work made clear the importance of accurate absolute determinations of the velocities in an electric field. Such determinations were meanwhile being carried out in the Laboratory by Zeleny (1901), who used a modification of the

method which he had already employed in his measurements of the ratio of the velocities of the positive and negative ions. The velocity of the ions in a known electric field was compared with the velocity of a current of the gas under examination—the electric field being at right angles instead of being parallel to the gas stream, as in the earlier experiments. Zeleny found, in accordance with the similar effect observed by Townsend in the case of diffusion, that moisture diminishes considerably the velocity of the negative ion, the loss of mobility of the negative ion being, as Zeleny points out, in all probability related to its property of acting more efficiently than the positive ion as a nucleus for the condensation of water vapour.

Using Townsend's diffusion numbers and applying his method of calculating *Ne*, Zeleny obtained fairly good agreement with the value deduced from the electrochemical equivalent of hydrogen. The value for the positive ions in the dry gases (with the exception of CO) came out considerably higher, though falling far short of twice that found for the negative ions. In the light of Townsend's recent experiments, this may be interpreted as being due to the complex character of the ionising radiation, a mixture of the two kinds of positive ions, associated with the single and double charges respectively, being produced.

The large volume of gas which is required restricts considerably the application of the method used by Zeleny to determine the velocities of the ions. It was during the period under review in this chapter that a former worker in the Cavendish Laboratory, P. Langevin, developed an extremely useful method, which is free from such restrictions. The method is described in a very valuable contribution to the subject of ionisation in gases which formed his thesis for the degree of Doctor of Science in

the University of Paris, entitled 'Recherches sur les gaz ionisés.' This was published in 1902.

The electrical conductivity imparted to air and other gases by drawing them past an electric arc or over the surface of incandescent metals was studied by McClelland (1899), who had already investigated the conductivity in the hot gases from flames. The electrical properties of the gases were shown to be such as could be explained by the presence of ions of comparatively small mobility, the velocity in a given electric field varying largely with the conditions, but being always greater for the negative than for the positive ion. McClelland found that in the gas drawn over a platinum wire whose temperature was gradually raised positive ions began to appear as soon as the wire was visibly red, while the negative only appeared at considerably higher temperatures. The number of the negative ions imparted to the gas, however, was found to increase with the temperature much more rapidly than that of the positive, until at a bright red heat as many negative as positive were produced. The results obtained by McClelland threw light on several already known peculiarities in the electrical properties of incandescent metals.

McClelland investigated also the effect of lowering the pressure of the atmosphere surrounding the hot wire. At moderate pressures the phenomena were such as could be explained by taking into account the ionisation by collisions, which Townsend had shown to be so important in the case of ionisation by x-rays at similar pressures. There was, however, evidence in the experiments at low pressures that a copious supply of negative ions escapes from the metal itself at high temperatures. The experiments were carried out at the Cavendish Laboratory, but they were not published till November 1901, after his appointment to

the Professorship of Natural Philosophy at University College, Dublin. His paper was read before the Cambridge Philosophical Society in November 1901. At the same meeting O. W. Richardson communicated the results of an investigation 'on the negative radiation from hot platinum'—the first of a long series of important researches on the electrical properties of hot wires, carried out first at the Cavendish Laboratory and continued after his appointment as Professor at Princeton University. Measuring the 'saturation' current at low pressures, Richardson found that the law of variation with temperature of the number of negative ions escaping from the wire was in agreement with the corpuscular theory of conduction in metals—a theory which had been discussed by Thomson in a Report to the International Congress of Physics at Paris in 1900.

The study of the conduction of electricity in flames, especially flames containing salt vapours, was taken up by H. A. Wilson in a series of investigations begun in 1898 (when he published a paper on the subject with Prof. A. Smithells and H. M. Dawson) and continued after his appointment as Professor of Physics at King's College, London. In his earlier investigations he was led to conclude (in opposition to the views held by Arrhenius and other investigators) that the ions in flames are produced only in the immediate neighbourhood of the glowing electrodes. It was not till 1905, when he studied the distribution of the field in a flame through which a current was passing—a quartz tube burner specially constructed for the purpose making it possible to perform the experiments under very definite conditions—that the matter was cleared up. It followed from these experiments that the conductivity is due to ionisation throughout the flame, and that salts are ionised in whatever part of the flame they are placed, not merely close to the glowing electrodes. He now found the

true explanation of the many experimental results which his earlier view had seemed to account for in the fact that the electric force in a flame traversed by a current is very small, except in the immediate neighbourhood of the electrodes, where it is large.

A great advance in the theory of conduction in ionised gases was made by the publication of Thomson's paper on 'The Theory of the Conduction of Electricity through Gases by Charged Ions' in March 1899. Towards the end of the same year Thomson proved that the ratio of the mass to the charge for the negative ions set free at low pressures from metal plates exposed to ultra-violet light, as well as for the negative ions from a glowing carbon filament, is the same as the corresponding ratio for the cathode rays—a ratio which does not much exceed one-thousandth part of that of the mass of the hydrogen atom to the charge carried by it in electrolysis. He determined the charge carried by the negative ions escaping from a zinc plate exposed to ultra-violet light, applying the method of making the ions visible by the condensation of water upon them, which he had already used to determine the charge on the ions set free in a gas by x-rays. The value found for the charge agreed with that already found in the case of ionisation by x-rays, which later experiments, by himself and by H. A. Wilson, showed to be too high. Thomson's experiments proved that the charge carried by the ions from a negatively charged zinc plate exposed to ultra-violet light differs little from that carried by the hydrogen atom in solutions, and is probably identical with it. A more direct proof of the identity was furnished by Townsend's diffusion researches to which reference has already been made. Assuming the charge on the ions escaping from a negatively charged zinc plate exposed to ultra-violet light to be independent of the pressure, we conclude, from the identity of the charges and

the relatively small value of the ratio of the mass to the charge as compared with this ratio for the hydrogen atom in solutions, that when the pressure is low the negative electrification is associated with particles whose mass is very small—not much exceeding one-thousandth part of that of the hydrogen atom. We can have no hesitation in inferring that, in the other cases in which the ratio of the charge to the mass is the same as for the ions set free by ultra-violet light at low pressures, the mass of the carrier of the negative charge has the same value.

A very important discovery concerning conduction in a gas ionised by *x*-rays was made by Townsend in 1900. Working with a gas at moderately low pressures, and subjecting it to a gradually increasing electric force, he found, as others had done, that, while the current at first increased with increasing electric force, a stage was soon reached beyond which the current became independent of the electric force—reaching a 'saturation' value. But, still continuing to increase the electric force, he found that a further stage was reached beyond which the current increased rapidly with increasing electric force. Townsend described his experiments in a letter to *Nature* in August 1900, and showed that his experimental results could be quantitatively explained by secondary ionisation caused by collision of the negative ions with the gas molecules—the possession of a certain minimum kinetic energy by the negative ion being necessary in order that the collision should result in ionisation. The kinetic energy of the ion is the equivalent of the work done upon it by the electric force in the course of its free path—the kinetic energy due to temperature being negligible by comparison.

In a paper on 'The Genesis of the Ions in the Discharge of Electricity through Gases,' published in the *Philosophical Magazine* for September 1900, Thomson also attributed

to the same cause—i.e. the collisions, with the neutral molecules, of already existing free ions which had acquired the necessary kinetic energy under the action of the applied electric force—the ionisation met with in the spark discharge and in the ordinary discharge through a vacuum tube. He also, like Townsend, explained by its means some of the apparently anomalous results which had been obtained by Stoletow with ultra-violet light.

Townsend published a detailed account of his experiments and of the theory brought forward to explain them in the *Philosophical Magazine* for February 1901—immediately after his appointment to the Wykeham Professorship of Physics at Oxford. He was able to prove that the energy required to ionise a molecule in air is not greater than that acquired by an ion in travelling freely between two points differing in potential by five volts—a value extremely small compared with previous estimates. Townsend continued in Oxford the work begun in the Cavendish Laboratory in a very important series of investigations dealing with ionisation by collision due to the negative ions, and later also with that produced in the much stronger fields required to give the positive ions the energy necessary to cause ionisation by collision—the primary ionisation being in most cases produced by the action of ultra-violet light on a metal plate. Much light was thrown by these investigations on the nature of the spark discharge.

Strutt began his work in the Cavendish Laboratory in 1899 with an investigation of the least potential difference required to produce a spark in various gases. When the conditions are suitable, fairly consistent values can be obtained for the potential difference required to cause a discharge, depending of course upon the distance between the electrodes and the pressure of the gas. When the pressure is varied there is, for a given distance between

the electrodes, a certain pressure for which the sparking potential is a minimum. Now, while the minimum value of this pressure depends on the distance between the electrodes, the same minimum value is found for the sparking potential difference for a wide range of variation of the distance between the electrodes. The object of Strutt's experiments was to obtain sufficiently trustworthy measurements of the minimum sparking potential to make possible an exact comparison with Warburg's determinations of what is called the cathode fall of potential. Strutt concluded that the minimum spark potential is equal to the cathode fall; both are, however, in the case of certain gases, affected to an extraordinary degree by minute traces of impurity in the gas. This makes an exact agreement impossible. Strutt made measurements also of the cathode fall in helium and argon.

A number of investigations dealing with the electric discharge in gases at low pressures were conducted in the Cavendish Laboratory during the period under review in this chapter.

Willows (1900) investigated the conditions affecting the nature and distance apart of the remarkable striations of the positive column which are such a striking feature of the discharge at moderate pressures. Later both he and Almy (1900) studied the effect of a magnetic field upon the discharge, their work being an extension of that of Birkeland, who had found that, when a longitudinal magnetic field at the cathode has reached a certain critical value, it produces an extremely large diminution in the potential difference between the electrodes. Both Almy and Willows found that under certain conditions a transverse field produces similar effects.

H. A. Wilson (1900) measured the electric intensity and the conductivity in the various parts of the electric discharge in

a rarefied gas. What is known as the Hall effect—the production, by a transverse magnetic field acting on a conductor conveying a current, of an electromotive force at right angles to the main current—had long been known in the case of metallic conduction, and it admitted of explanation on the ionic theory of conduction in metals. On account of the large difference between the velocities of the positive and negative ions such an effect was to be expected in gases. Wilson (1901, 1904), who had now succeeded Townsend as Clerk Maxwell Student, investigated quantitatively this effect in the positive column.

H. A. Wilson (1902) investigated also the current density at the cathode in the electric discharge in air. It was known that, when the pressure lies within certain limits, the negative glow surrounding the cathode is confined to a definite area depending on the magnitude of the current, the area increasing as the current is increased. Wehnelt had shown that the discharge from the cathode is confined to the part covered by the glow, and that the current density is uniform throughout this area. Wilson found that the area covered by the glow is proportional to the current till the cathode is entirely covered. He measured the current per square centimetre of the gas under various conditions.

Burke (1901) investigated the phosphorescence produced in gases by the electrodeless ring discharge; he found that the phosphorescent glow is propagated along a tube attached to the discharge bulb with quite a small velocity—about two metres a second.

Several investigations on cathode rays were completed during this period.

Strutt in 1899 showed that it is only when it is produced by a variable source of potential, such as an induction coil, that a beam of cathode rays is spread out by a magnetic field, giving rise on the walls of the tube to the 'magnetic

spectrum' of Birkeland. When the cathode rays are produced by a battery of storage cells he found that all the cathode rays are equally deviated by a magnetic field. He concluded that when a magnetic spectrum is produced, the conditions are such that it is due to a variation in the velocities of the cathode rays produced at different instants on account of the varying electric force, and that there is no need to assume any variation in the masses or charges of the cathode ray particles.

H. A. Wilson also investigated (in 1901) the magnetic deflection of cathode rays, proving more completely than had hitherto been done that the ratio of the charge carried by a cathode ray particle to its mass is independent of the metal of which the cathode is composed.

McLennan, at that time Demonstrator of Physics in the University of Toronto, now Professor there, investigated the conductivity in gases traversed by cathode rays, the results being published in a paper read before the Royal Society in December 1899. He used a double apparatus in which air and another gas could be exposed to identical beams of Lenard rays. The rapid absorption and scattering of the rays by gases at other than very low pressures makes it extremely difficult to compare the ionisations in gases exposed to cathode rays of equal intensity. McLennan was able to avoid this difficulty completely by adjusting the pressures so that the densities of the gases compared were equal—the intensities of the radiation at corresponding points of the two identical ionisation chambers being then the same, in consequence of the simple absorption law discovered by Lenard. McLennan found that in all the gases studied equal ionisations were produced by cathode rays when the densities were made the same.

On a second visit to Cambridge, in 1901, McLennan discovered and investigated a peculiar effect of cathode rays

upon certain salts. These, after being exposed to cathode rays (or to the rays from a spark discharge), were found to have stored up in them the power of emitting negative ions on being slightly heated.

Durack (1902) investigated quantitatively the ionising power of Lenard rays, measuring both the charge carried by a pencil of the rays and the ionisation produced by it in traversing a known thickness of air at a definite pressure. He found that in air at a pressure of 1 mm. of mercury each Lenard ray corpuscle produced about 0·4 pairs of ions for each cm. traversed. Townsend had found that a very much larger number (about 20 per cm. for a pressure of 1 mm.) are produced by the much more slowly moving ions, which are effective in ionisation by collisions —the velocity in this case being only about one million metres per second as compared with forty millions in Durack's experiments. Durack found a still smaller number for the much more rapidly moving β-ray corpuscles.

Strutt investigated in 1900 the ionisation produced in various gases by the Becquerel rays. At this time, while the β rays were known to consist of negatively charged corpuscles, the nature of the much more readily absorbed α rays was not understood. Strutt found that α and β rays resemble one another as regards their ionising effect on gases much more than they resemble x-rays. He threw out the suggestion that the α rays might possibly turn out to be positively charged particles and that a sufficiently powerful magnetic field might cause a deviation—in the opposite direction to that of the β rays; the truth of his suggestion has been established by many subsequent researches.

It was shown almost simultaneously (in November 1900), in Germany by Geitel and in the Cavendish Laboratory by C. T. R. Wilson (who was at this time working at Atmospheric Electricity for the Meteorological Council), that a charged

body suspended within a closed vessel gradually loses its charge even when precautions are taken to avoid all possibility of leakage over the insulating supports; and that the leakage through the air in the vessel shows all the features characteristic of conduction through an ionised gas. Wilson concluded from his measurements that about 20 ions of either sign were produced per second in each cc. of a small ionisation chamber (of glass silvered internally) which contained 163 cc. of air at atmospheric pressure. (The number would be nearly doubled if the more recent value of the charge of the ion were employed.) To test whether the ionisation might be due to a very penetrating radiation traversing our atmosphere, Wilson in the following vacation compared the ionisations in a closed vessel when carried into a railway tunnel with that in the same vessel placed in an ordinary room; no indication of any absorption of such rays was observed. Experiments, made in the following year, on the 'spontaneous' ionisation in various gases at different pressures gave results showing a remarkable resemblance to those obtained by Strutt with the same gases exposed to Becquerel rays. This suggested the possibility that the 'spontaneous' ionisation might really be wholly or partially due to materials, such as those composing the walls of the vessel, being to a slight extent radio-active. Patterson working with air in a large ionisation vessel at different pressures concluded that the ionisation is due to an easily absorbed radiation from the walls. It was proved by the work of Strutt, McLennan and Burton, and Cooke that the ionisation is due partly to radiation from the walls of the vessel, partly to a much more penetrating radiation which traverses the walls of the vessel. The subject of the radio-activity of ordinary materials belongs rather to the period reviewed in the next chapter; it has been studied with special thoroughness by Campbell.

The penetrating radiation is probably due to the presence of radium and its products in the earth and in the atmosphere. Two examples illustrating the wide distribution of these bodies belong to this period.

Some remarkable effects obtained when air was bubbled through water were noticed by J. J. Thomson (1903) and were finally traced by him to the presence of a radio-active emanation in the Cambridge tap water. Adams (1903) proved that the emanation was identical with that of radium.

It had been proved by Elster and Geitel that a negatively charged body exposed in the atmosphere becomes temporarily radio-active, indicating the presence of a radio-active emanation in the atmosphere. This suggested to C. T. R. Wilson (1902, 1903) that freshly fallen rain and snow might be radio-active, and this was found to be the case.

J. J. Thomson (1901) found no evidence of any ionisation as a result of the gradual combination of hydrogen and chlorine under the influence of light. Nor did the production of ions in the mixed gases by means of x-rays have any effect on the rate of combination.

Bevan (1901) carried out a very complete investigation of the whole subject of the combination of chlorine and hydrogen under the influence of light.

Experiments on secondary x-rays were made by Townsend in 1899. He showed that two kinds of secondary rays are emitted by solid bodies on which x-rays are incident—a fairly penetrating kind (previously studied by Sagnac) and a very easily absorbed radiation to which the surface effect discovered by Perrin is to be attributed. Similar experiments had already been made in the Cavendish Laboratory by Langevin, but they were not published till 1902.

Barkla had already begun his important work on secondary x-rays before leaving the Cavendish Laboratory in 1902 to become 'Oliver Lodge Fellow' in Liverpool. He had

previously worked at the velocity of electric waves along wires, investigating the influence of the diameter and material of the wire upon the velocity with the aid of Rutherford's magnetic detector.

Experimental investigations relating to electrical conduction in metals, suggested by the theory put forward by Thomson in his report on the Constitution of Matter to the International Congress of Physics at Paris in 1900, were undertaken by W. C. Baker and by Patterson (1902). Baker investigated the Hall effect, Patterson the change in the resistance of metals due to a magnetic field, and the variation of the specific resistance with thickness in thin metallic films. Thomson had pointed out (*Camb. Phil. Soc.*, March 1901) that when the thickness of a metallic film is reduced below a certain value (equal to the mean free path of a corpuscle in the metal), a very rapid increase of specific resistance is to be expected. Patterson found that platinum films subjected to a definite treatment had a specific resistance practically independent of the thickness until this was reduced to about 7×10^{-7} cm.; for films thinner than this the specific resistance increased rapidly as the thickness was decreased.

The investigations we have thus far considered may nearly all be regarded as being more or less directly connected with the conduction of electricity through gases, or as having been suggested by ideas which owed their origin to the advances in our knowledge of that subject. Some researches in other directions remain to be considered.

Whetham continued to work at solutions, measuring (1900) the electrical conductivity of aqueous solutions at the freezing point of water. If the measurements are made at this temperature the conductivity determinations become directly comparable with freezing-point data for the same solutions. Special precautions were taken to obtain reliable results for

dilute solutions; to avoid as far as possible all risk of contamination of the solutions the whole apparatus was made of platinum.

Searle and Bedford (1901 introduced a new method of measuring magnetic hysteresis. A single hysteresis measurement by the ordinary method of plotting a complete B and H curve is a comparatively long and tedious process. In the method used by Searle and Bedford a measurement of hysteresis can be effected almost instantaneously by observing the throw of a ballistic electrodynamometer. On account of the speed with which the measurements can be made by this method and of the ease with which it lends itself to simultaneous measurements of magnetic induction and hysteresis, it can be applied to investigations which would be quite impracticable by the ordinary method—investigations, for example, of the effects of temperature or of strain on hysteresis. Several interesting investigations on the effects of strain were made by Searle and Bedford. Wills (1903) used the method to investigate the effect of temperature on the hysteresis loss in iron.

The effect of temperature on the magnetic susceptibility of iron and certain alloys was studied by S. W. Richardson and by Wills, who both published papers in 1900. Richardson confined himself to aluminium-iron alloys, which he studied over a wide range of temperature from — 83° C. to +900° C.

There remain to be considered some investigations dealing with the mechanical properties of matter.

Vincent (1898) obtained a large series of photographs of ripples illustrating in a very instructive way the propagation of waves and their reflection, refraction and diffraction. He also made experiments with a mechanical model illustrating modern theories of dispersion of light. The model imitated very closely the behaviour of a substance showing 'anomalous'

dispersion. A research of a very different kind was that completed by him in 1901 in which he made extremely accurate measurements of the density and cubical expansion of ice.

The important application by Townsend, Thomson and H. A. Wilson of Stokes' equation giving the rate of fall of small spheres in a viscous fluid made it desirable to test the range of its applicability experimentally. H. S. Allen (1900) measured the terminal velocity of small air bubbles and of various solid spheres in water both within and outside the limits of application of Stokes' formula.

In elasticity Searle (1900) introduced some novel and interesting methods of measuring elastic constants. Shakspeare (1899) made very accurate measurements on Young's modulus by an interference method and studied the effects of temperature and magnetisation.

From a Photo by Stearn, Cambridge]

RESEARCH STUDENTS, 1909:—

L. VEGARD J. STEAD J. R. WILTON H. H. PAINE G. I. TAYLOR
H. THIRKILL R. WHIDDINGTON L. SOUTHERNS J. M. ADAMS E. M. WELLISCH C. S. WRIGHT R. T. BEATTY
G. W. C. KAYE T. H. LABY H. ERIKSON W. BORODOWSKY H. V. GILL D. M. BOSE
J. CROWTHER F. HORTON D. B. PEARSON SIR J. J. THOMSON E. R. LAIRD N. R. CAMPBELL J. SATTERLY
A. LL. HUGHES J. A. ORANGE R. KLEEMAN

CHAPTER VIII

(1903–1909)

THE present day is a period as difficult in history as in biography. The absence of perspective tends to invest all details with the same importance, and the task of selecting those which will be of interest to a reader after the lapse of even ten years is almost hopeless. The difficulties are not diminished by the recent increase in the number of those working in the laboratory and, consequently, of the number of events which claim record. During the period to which our attention is now directed the average number of research students present during one year was about 25; in the preceding period it was only 12, and in earlier days it was smaller still. An apparent vagueness and incoherence in the work performed is a natural result of the increase in its quantity, but it may be hoped that to those whose view is retrospective the decline in quality will be seen to be merely apparent and due to the wider range of subjects and the difficulty of grouping them round a few central ideas. On these grounds we excuse ourselves for the omission of any general summary, and for proceeding directly to the narration of individual events.

Of these events those of a personal nature first claim our notice. There is a long record of distinctions gained by those who have worked in the Laboratory. Three times in seven years have they gained one of the magnificent prizes

founded by Nobel for annual award to those who, irrespective of nationality, should have performed the most notable achievements in the development of physics, chemistry, medicine or literature, or in the promotion of international peace. In 1904 Lord Rayleigh, in 1906 Professor Thomson, in 1908 Professor Rutherford received a prize. In the two cases first mentioned the prize was for physics, and the joy with which the announcement was received could be accompanied by no expression of surprise; but, when the prize for chemistry was allotted to Professor Rutherford, our satisfaction at the award was enhanced by the unexpected recognition by the students of that science of the great services which might be rendered to them by researches of purely physical origin.

In April 1908 the Chancellorship of the University became vacant by the death of the eighth Duke of Devonshire. In no branch of pure learning has Cambridge reason to fear comparison with any other institution, but the work of her physical school during the past century is perhaps her most incontestable claim to supremacy. It was fitting, therefore, that the son of the munificent founder of the Cavendish Laboratory should be succeeded in the highest office of the University by one of its three distinguished professors. Lord Rayleigh was elected Chancellor, and one of his first official acts was to open the new buildings (see Chapter I.) which he himself had presented.

In 1908 Professor Thomson was elected President of the British Association for the Advancement of Science for the meeting in Winnipeg in August 1909. Shortly afterwards it was announced that he was about to receive the honour of knighthood. The gratification of his friends, colleagues and pupils that he should be thus selected for special distinction was perhaps mingled with amazement that in the official mind no distinction could be made between

achievements which had received recognition in every civilised country and those of many a provincial mayor.

A list of new Fellows of the Royal Society is rarely published without the inclusion of the name of at least one member of the Cavendish Laboratory. In 1903 Professors Rutherford and Townsend were elected, the former for his discoveries which founded the science of radio-activity, the latter for his work on the ionisation of gases. In 1904 the same honour fell to Mr. (now Professor) Newall, who had resigned in 1890 the Demonstratorship of Physics and devoted himself to astrophysical research; in 1905 to Mr. (now Professor) Strutt, whose investigations on the distribution of radio-active elements in the soil seem likely to influence greatly the development of geological science; and to Mr. Searle, the senior Lecturer and Demonstrator in the Laboratory, for his work on magnetic measurements and electromagnetic theory; in 1906 to Professor H. A. Wilson for his investigations on ionisation in flames and many other subjects; and in 1909 to Professor McClelland for his work on β-rays. In 1905 Rutherford also received the Rumford Medal.

The stream of men trained in the Laboratory to fill important posts in other learned institutions has flowed undiminished. H. A. Wilson left us to take up first a lectureship and then the professorship at King's College, London, and later a professorship at Montreal. O. W. Richardson was elected to a professorship at Princeton, New Jersey; Durack, Bevan, and Laby to similar posts at Allahabad, Holloway College, and Wellington College, New Zealand. Skinner resigned his demonstratorship for the post of Principal of the Chelsea Polytechnic Institute. Patterson, Field, and G. W. Walker were appointed to important meteorological posts, the first two at Simla, the last at the newly established Eskdalemuir Observatory.

Other appointments fortunately made no change in the personnel of the Laboratory. Professor Thomson became professor at the Royal Institution on the resignation of Lord Rayleigh, and Fitzpatrick, one of our oldest teachers, became President of Queens' College. It must also be mentioned that Professor Rutherford returned from Canada to Manchester, where he is founding a school of research that may rival or even eclipse the fame of its parent; that Strutt became professor at the Imperial Technical College, M. Langevin professor at the Sorbonne; and that Newall was elected to a specially-founded professorship of astrophysics in the University of Cambridge.

Notwithstanding the departures caused by the filling of these and other appointments, the Cavendish Laboratory gained almost as much as it lost during these years. We were so fortunate as to be favoured with the presence of a most distinguished group of physicists from abroad. Professor Bumstead from Yale, Professor Mackenzie from Brynmawr, Professors Nicholls and Hull from Davenport College, Professor More of Johns Hopkins, and Dr. Smoluchowski of Lemberg arrived in Cambridge between 1904 and 1908. Though none of these visitors could remain for more than a year, the influence of the new ideas which they imported and the new experimental methods which they taught was of immense and permanent value.

At our request Professor Bumstead has been so good as to write us a letter in which he gives some account of the impressions made by a visit to the Cavendish Laboratory on one who is accustomed to universities of a totally different nature and organisation. It is, perhaps, to be regretted that his courtesy has led him to insist upon the virtues rather than upon the defects of Cambridge, but we venture to think that the following quotations will be found of some interest:

It is difficult for me to give you anything like an adequate account of my impressions of Cambridge and the Cavendish Laboratory, partly because it is a pretty complex social organisation, and partly because I was so enthusiastically pleased with it that I am in danger of falling into what may sound like indiscriminate eulogy.

With regard to the University as a whole, one thing which struck me was the essentially British way in which it has utilised survivals of the past: not sweeping them away because they were or might become abuses, but adapting them to modern conditions in such a way that you have a better instrument for the presentation and enlargement of knowledge than we can make here with the ground all clear for the most 'modern improvements.'

Two illustrations of what I mean will suffice. One is the division of the University into colleges, which seems to me of enormous advantage to the social life of both undergraduates and dons and to their broad intellectual development. In this country we have schools of engineering, law, medicine, divinity, the liberal arts, and so on: the social groups tend to follow these lines with a distinctly narrowing influence. I think you have a much better arrangement, and yet I cannot see how it could be made to order: it is only history and tradition that make Trinity and St. John's and Caius all different and yet parts of one whole.

Another survival which I envy you is the system of fellowships, which, I suppose, was the source of much abuse in the past and may be even now liable to some abuse. But I believe it is on the whole the best means of promoting research and sound scholarship which could be devised. Only it could not be devised: I cannot imagine even the most enlightened millionaire 'throwing away' his money on such a scheme. Modern benefactors seem always to be in favour of 'small profits and quick returns:' they want to see results at once. . . . If you can keep your fellowship system from being reformed too much, you will be fortunate among the universities of the world.

The whole social atmosphere of Cambridge is to an American university man most charming and delightful. Its simplicity and lack of ostentation we have kept pretty well over here, possibly as a direct inheritance. But your social life struck me as much richer and fuller than ours: really we seem too busy to enjoy each other's society, and yet we do not get so much done as you. I don't understand it altogether: a laboratory in this country in which nobody ever began work before 10 A.M. or

worked later than 6 in the evening[1] would serve as a terrible example of sloth and indolence. I do not see how you get so much work done and yet have time to live so pleasantly and unhurriedly. We Americans have never discovered that great secret.

As for the Laboratory itself, it is difficult to describe the (almost paradoxical) combination of qualities which I thought I observed there. It is obviously dominated by the personality of 'J. J.': and yet I have never seen a laboratory in which there seemed to be so much independence and so little restraint on the man with ideas. The friendliness and mutual helpfulness of the research students was obvious and one of the finest things about the place, and it appeared to be a part of this friendly service to jump into a fellow-student, if you thought him wrong, and to prove him wrong. In a good many places friendship does not stand that strain, but it usually does at the Cavendish.

Another thing which struck me as paradoxically fine was the relation between the Professor and his students. The great admiration and reliance with which 'J. J.' was regarded by everybody was unquestionable: yet in matters of detail there was no subserviency. I saw a good many men, while I was there, following their own courses, and I remember well the frank tone prevailing at the meetings of the Cavendish Society, where the Professor's theories or experiments were not spared in the general criticism. . . .

The Editors have suggested that, in giving some account of the research carried on in the Cavendish Laboratory, the ideas from which it sprang should be traced as far as possible. The suggestion is doubtless far better than that of any chronological arrangement, but it renders historical accuracy difficult, and leads to the close connexion of investigations which may have been widely separate in the minds of their authors. We would state, therefore, explicitly that we do not profess to describe the train of thought by which the various researches were developed, but only their relation as they appear in a general and impersonal review.[2]

[1] Prof. Bumstead has, perhaps, exaggerated slightly our 'sloth and indolence'!

[2] It should be pointed out that no consistent attempt is made to follow researches performed outside the Cavendish Laboratory, even though they

But before considering the ideas which were prevalent in the period with which this chapter is immediately concerned, we must notice some work which, though performed in this period, belongs logically to that which preceded. The problems characteristic of the era which ends when this chapter begins were those of the mechanism of the conduction of electricity through gases. Between 1895 and 1903 the more important of these problems had been solved; it had been shown that the current was carried by charged particles of either sign, and many of the properties of these particles had been discovered and recorded. Incidentally the negative electron or 'corpuscle,' the electrical unit common to all bodies, had been detected experimentally and many of its properties measured. But, of course, investigations in this direction were not, and are not, ended: they have continued up to the present moment. Two remarkable pieces of work of this nature claim special attention.

The first of these is the redetermination of the charge borne by an electron, carried out by Professor Thomson (1903) and by H. A. Wilson (1903), which has been described in its proper logical order in the previous chapter. The values obtained were 3.4×10^{-10} by Thomson and 3.1×10^{-10} by Wilson. It may be noted that it seems certain nowadays that both of these estimates are too low: the most reliable measurements made during the past few years by various workers, using different methods, direct and indirect, agree in making the electronic charge between 4.1 and 4.7×10^{-10}.

The other piece of work to which reference was made concerns the nature of the ions which carry the current in gases at high pressure. The measured mobility of these

anticipated or extended work done within its walls. The inclusion of such work would necessitate the writing of the complete history of physics during our period, and is impossible for reasons of space.

ions appeared to indicate that they were rather larger than the molecules of the gas in which they were formed, and it was commonly believed that they consisted of a cluster of two or three molecules collected round the original charged particle, as fragments of paper collect round a charged rod. But the estimate of the sizes of the ions, based upon different experimental and theoretical considerations, such as are given in the memoirs of McClung (1904) and Richardson (1905), led to conflicting conclusions. The most hopeful way of resolving the doubts thus raised appeared to be by the comparison of the mobility of the ions in a mixture of gases with that of the ions in either of the pure gases: if the ions consist of a cluster of several molecules there should be an almost continuous gradation of ions between the extremes represented by the two pure gases. Many efforts to conduct such measurements have been made during the past five years, but the experimental difficulties proved insuperable, until the problem was attacked by Wellisch (1909). By the combination of ingenious mathematical investigation with skilful experiment, he has succeeded in showing that the ions really consist of only a single charged molecule, and that their apparently greater size is due to the inductive attraction which they exert on neutral molecules and to the consequent increased frequency of collision. He has established also the remarkable fact that the charge on the molecule of one of the two mixed gases can be transferred to a molecule of the other without the intervention of any ionising agent, a conclusion of the most startling nature and one that may affect fundamentally our views of the nature of ionisation.

Among the other researches which continued work characteristic of preceding periods may be mentioned Phillips' measurement of ionic velocities at different temperatures (1906) and Erikson's measurement of the

coefficient of recombination of ions (1909). C. T. R. Wilson (1904) continued his investigations on Condensation Nuclei, which were extended to liquids other than water by Laby (1908).

We must now turn to the work which may be regarded as based upon ideas specially characteristic of our particular period.

The preceding period may be termed the 'electronic or (to use the name more often employed in Cambridge) the 'corpuscular' period. A new concept had been introduced into physics, that of the universal negatively charged electron, which had been brought from the region of theory into that of experiment: the magnitudes of the few simple qualities attributed to it had been measured. The next step was clearly to apply the new knowledge to the fundamental problem of modern physics, the relation between electricity and matter—to attempt, that is, to devise with the aid of these bodies a mechanism which should represent the electrical properties of material systems, just as, by the aid of the molecule, a mechanism was devised to represent their thermal properties. But before setting out on such an endeavour, it was desirable to take as wide a retrospect as possible over the lands which had been traversed, to review the work which had been done and the information which had been gained. The appearance in 1903 of Thomson's monumental treatise on 'The Conduction of Electricity through Gases' could not have been more opportune. Not only as a summary of a large and scattered body of facts, but as a complete and logical statement of their significance in the view of one of the most important investigators, it has been the inspiration of much subsequent research.

The inception of the new advance was marked by the

appearance throughout the physical world of many electrical theories of matter. It was natural that those which exerted the greatest influence at the Cavendish Laboratory were originated by its Professor: Thomson's papers on the structure of the atom, its magnetic, optical and conductive qualities (1903, 1904, 1906), the substance of which appeared later in a less technical form in his 'Electricity and Matter,' and his 'Corpuscular Theory of Matter,' may be said to summarise the main ideas from which the research of the period sprang. In these publications the author showed that, by assuming for material bodies a structure no more complicated that the most incontestable experiments rendered necessary, by making no more elaborate hypothesis than that the atom contains a finite number of corpuscles, the charge on which is neutralised by an equal positive charge of the simplest distribution, a very large number of previously unconnected physical and chemical phenomena could be correlated and explained. Electrical and thermal conduction, magnetism, radio-activity, the refraction and emission of light, and the chemical changes that need the conception of valency were the chief groups of facts which were brought for the first time, even tentatively, within the range of a single theory.

This theory had now to be tested by investigating whether any of the properties which must be possessed by such a hypothetical structure, other than those which were known already and had been used in its invention, could be observed experimentally. Unfortunately no very direct tests could be applied, for the quantitative prediction of the properties of the structure was impossible. The atom is a very complex piece of mechanism, and it was clear from the outset that it must either contain a number of electrons too large to be amenable to mathematical investigation, or include some portion other than the corpuscles, of which nothing

was known as yet. Nevertheless, there are three lines of research carried on in the Laboratory which may be considered for our purpose, if not with complete historical accuracy, as tests of the corpuscular theory of matter.

The first of these lines is that for which its chief exponent has recently proposed the name 'Thermionics.' According to the electronic theory the conduction of electricity in metals is effected by the motion of electrons which are free temporarily from the atoms of which they form part. In order to account for the intimate connexion between electrical and thermal conduction, it is necessary to suppose further that the same electrons are the chief agents in heat conduction and share, in accordance with established principles, in the motion of agitation of the molecules, which represents the temperature of the body. Now it is known that, if the temperature of a body be raised sufficiently, the speed of agitation of the molecules becomes so great that they break free and cause the liquefaction or evaporation of the substance: and it might be expected that, if the electrons share in the agitation, they should also break free, if the metal is hot enough, and give rise to an electric current from the metal to surrounding bodies. Phenomena had been observed previously which might be attributed to this cause, and Richardson (1903, 1904, 1905, 1906) proceeded to investigate the matter more nearly. His researches, continued at Princeton, and those of H. A. Wilson (1903), Owen (1903, 1904), and Horton (1907) (together, of course, with much work done outside Cambridge), have shown clearly that the current flowing from a red or white hot body is in all essential respects in accordance with the prediction of theory: by measuring the velocities of the electrons emitted, Richardson and Brown (in work done at Princeton) have succeeded in producing the first direct experimental proof of the well-known

Maxwell-Boltzmann law of the distribution of velocities among a system of bodies moving similarly to the molecules of a gas. Many facts of high interest have been discovered incidentally, such as the great influence which the chemical composition of a conductor exerts upon the ease with which electrons escape from it, and the existence of a current of positive electricity from the metal co-existing with the negative current, but masked by it at higher temperatures. 'Thermionics' seem likely to develop into a branch of physics with almost the same importance as radio-activity.

Other work connected with the theory of conduction is represented in the papers of Laws (1903, 1904), Blyth (1903), and Horton (1906); but a detailed consideration of it would lead us too far from the central problem on which our attention is now fixed.

At the end of 1902 the new science of radio-activity was founded by the publication of the theory of Rutherford and Soddy: it might be thought by the general reader that so notable an event would exert an overwhelming influence on the work of the period under consideration. As a matter of fact, the influence of the discovery of radio-activity upon work at the Cavendish Laboratory has been almost entirely indirect. Some distinctively radio-active investigation has been carried out by Makower (1905), Levin (1907), Slater (1905), Mackenzie (1905), Huff (1906), and Logeman (1906), but for the most part the new phenomena have only been studied with the view of solving the fundamental and long-standing problems which were independent of their discovery. Even the question of the possibility of a general radio-activity of all elements, to which we must now turn our attention, is more aptly regarded as arising from theories of atomic structure than from the special ideas peculiar to the science of radio-activity.

It may be well to remind the reader who is not a

professed student of physics that radio-activity consists in the spontaneous emission of rays, consisting, at least in part, of charged particles travelling with high velocities and capable of ionising a gas or affecting a photographic plate: it is distinguished from other phenomena to which it shows some resemblance by the spontaneity of the emission and the fact that it is accompanied by the production of new elements in the substance from which the rays are emitted. The only explanation of the phenomenon which has met with any success regards the rays as portions of an atom ejected in consequence of a sudden collapse of that structure. The instability which gives rise to the explosion is regarded as following from the slow, but constant, loss of energy which must be suffered by electrons moving in closed orbits within the atom: since a given orbit can only be described by a particle the speed of which lies within certain limits, the decrease of speed below the lower of these limits would necessitate a sudden change in the constitution of the atom, which would be accompanied by events similar to those observed in radio-active bodies. Now the presence of such electrons moving in closed orbits is regarded as connected intimately with the optical and magnetic properties of the atom: and in respect of these properties the highly radio-active elements do not differ markedly from the other elements. Accordingly theory would lead us to suspect that the difference between the two classes is one of degree and not of kind, and that, if the common elements were examined with sufficient care, some traces of the spontaneous emission of ionising rays might be found.

The likelihood that such an examination would give a positive result was much increased by observations by several investigators—of whom C. T. R. Wilson, Strutt, and McLennan may be mentioned as past or present members

of the Laboratory (see Chap. VII.)—which proved that a gas was never completely free from ionisation, but always contained a few ions which appeared to be formed under the action of rays, similar in many respects to radio-active rays, proceeding from the walls of the containing vessel. It was also shown that the intensity of these rays depended in some measure on the material of which the walls were composed: the ionisation in a lead vessel, for example, was always greater than that in one of aluminium. It appeared highly probable that the emission of these rays might be due to an intrinsic rádio-activity of these metals.

However, it soon appeared that the question whether radio-activity was a general property of the elements was not to be settled in so simple a manner. Many experiments, including those of Thomson (1903, 1904) and Adams (1903) at the Cavendish, proved that, though radium and its descendants were extremely rare elements if tested by the proportion contained in any mineral of natural occurrence, they were extremely common elements if tested by the wideness of their distribution. Almost all soils and rocks, most forms of natural water, and the atmosphere itself have been shown to contain measurable quantities of some of these ubiquitous elements. The possibility had to be considered that the ionising rays from common elements might be due, not to an intrinsic radio-activity, but to the presence of a minute trace of highly radio-active impurity, or might represent a 'secondary radiation,' such as is known to be produced at the surface of a solid struck by radio-active rays, excited by the radium in the air or the earth.

Attempts to discriminate between the various sources of rays and to discover whether any portion of the ionisation could not be attributed to the presence of highly radio-active impurity constitute a considerable portion of the research done at Cambridge during our period. The chief workers

in this field were Campbell (1905, 1906, 1907), Cooke (1905), Jaffé (1904), Patterson (1903), and Wood (1904, 1905). Their methods included endeavours to cut off the external radiation by absorbing shields, in the use of vessels of different materials, or the same material subject to different treatment, and in the observation of vessels of different shapes and sizes, with the object of discovering whether the penetrating power of the rays from different materials was the same. The results attained in the Cavendish were, on the whole, favourable to the hypothesis of general radio-activity, and Campbell attempted to show that a difference in the quality of the rays from different common elements precluded the possibility of a common source. But the observations were exceedingly difficult, owing to the smallness of the ionisation involved, and investigations outside Cambridge have tended decidedly against the hypothesis. It cannot be said that there is any balance of evidence to show that any but a small group of elements are radio-active: perhaps it is only the difficulty of proving a definite negative that permits some of us to regard the question as still unsettled.

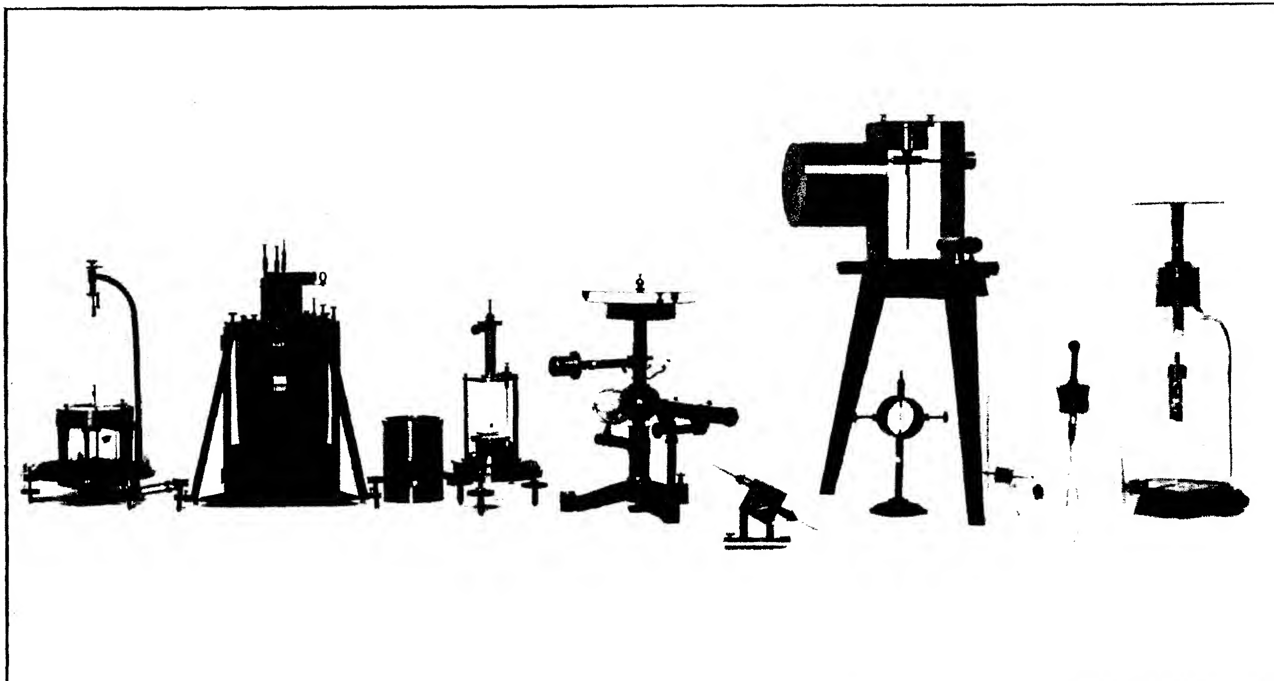
But though this line of research has not led to any definite conclusion, it has led to some bye-products of the highest interest and importance. Not the least notable of these is the invention by C. T. R. Wilson of the several admirable electroscopes (1903, 1905) for the accurate measurement of small quantities of electricity, which have aided so greatly in almost every investigation pursued at the Cavendish Laboratory. A specimen of the 'tilted electroscope,' which is to be found in almost every piece of apparatus, is represented by the small instrument in the foreground of Plate facing next page. Immediately on its left in the same plate is Wilson's portable electroscope for measurements of atmospheric electricity, while the round flask on

the right shows a rough form of the earliest instrument used by Wilson in his work on 'spontaneous ionisation.' The three instruments on the left of the photograph are electrometers arranged in chronological order. The oldest (on the left) is the original Kelvin quadrant electrometer: on its right is the improved Kelvin-White instrument, while on the right again is the modern Dolezalek electrometer. On the extreme right is one of the earliest instruments for measuring electricity, a rough gold-leaf electroscope used by Wollaston; the remaining electroscopes are the more modern forms of Curie (above) and Exner (below).

Investigations of 'spontaneous ionisation,' together with Wilson's work on Condensation Nuclei, may be considered to have led to researches on the electrical properties of the atmosphere, which have an important meteorological significance, such as those of Wilson himself (1903, 1905, 1908) and of Satterly (1908).

Wood and Campbell (1907) touched on similar questions when, in their endeavour to analyse the sources of disturbing variations in the 'spontaneous ionisation,' they detected and recorded a distinct diurnal periodicity in the intensity of the penetrating radiation from the earth and air. They also (1906) discovered by accident that the widely distributed element potassium and its congener rubidium emit spontaneously ionising rays of considerable penetration. The claims of these elements to be regarded as radio-active would be incontestable, were it not for the failure of all attempts to detect the production from them of any new element.

A third line of research which may be connected with the theory of atomic structure concerns the absorption of rays of various kinds and the secondary radiation excited by them: but it will be pointed out below that it has another and perhaps more important aspect. The chief examples of this work are the memoirs of Crowther on the absorption of



THE EVOLUTION OF THE ELECTROMETER

β-rays, and on the ionisation and secondary radiation produced by Röntgen rays in gases (1906, 1907, 1908, 1909); of Kleeman on the absorption and secondary radiation of the β and γ rays of radium (1907, 1908, 1909); of Laby on the total ionisation due to the α-rays of uranium (1907), and of Kaye on Röntgen rays (1907, 1908, 1909). Barkla's work on the last named subject, though performed at Liverpool, was the direct continuation of his work in the Cavendish Laboratory. Such investigations might be expected to be one of the most fruitful sources of information as to the minute structure of material bodies, for the processes of absorption, ionisation and secondary radiation are doubtless but various aspects of the interaction between the charged particles or undulatory disturbances constituting the rays and the nuclei of which the atom is composed. But the difficulty of applying mathematical treatment to any of the model mechanisms which have been proposed to represent the structure of the atom has prevented any direct application of the experimental results to the elucidation of this important problem. The relations which have been discovered are extremely suggestive, indicating an intimate connexion between the absorptive and the other properties of an atom, and proving the existence within it of structures possessing definite periods of resonant vibration much smaller than those which are concerned in optical phenomena: but the complete correlation of the varied collection of facts cannot be effected until many other grave questions have been answered. However the facts themselves are of the greatest interest and importance. A more detailed treatment of this branch of work, much of which ranks high among the achievements of the Laboratory, is omitted only because the great complexity of the results is at present incapable of summary analysis.

Another investigation which must be mentioned in connexion with the same ideas concerns the perennial mystery of physics, gravitation. Two hundred years have elapsed since the formulation of the theory of the universal attraction of material bodies, but it is still totally uncorrelated with any other theory. One of the best attempts at explanation suggests that the attraction between two electrically neutral bodies is due to a residual action of the opposite electrifications which they contain, so that the attraction between two charges of opposite sign is not equal to, but slightly greater than, the repulsion between two similar charges of the same sign. The plausibility of the suggestion is certainly increased by the recent discovery of the fundamental difference in the nature of positive and negative electricity. Thomson has pointed out that it is a consequence of this hypothesis (1909) that the ratio of the weight to the mass of a body should increase with the amount of electrostatic potential energy of its component parts. But, on the electronic theory of matter, the energy of radio-activity represents such electrostatic energy within the atom, and it might be expected accordingly that the ratio of the weight to the mass of an earlier member of a radio-active series should be greater than that of a later, derived from the earlier by the evolution of a large amount of energy. The experiments which Thomson undertook (1904) in accordance with this idea consisted of a repetition of the classical pendulum experiments of Newton with a pendulum of which the bob contained a salt of radium. The results were negative: the value of the gravitational acceleration for radium proved the same as that for any other substance within the limits of experimental error. But these limits were comparatively large, and at the present time the observations are being repeated in conjunction with Southerns, who has recently

concluded (1909), as the result of most delicate and skilful experiment, that the weight of a body is affected to some slight extent by the state of its electrification.

So far we have been concerned solely with research which might be interpreted on the supposition that the atom is composed of negative electrons, or corpuscles, and practically nothing else. But before the work had been completed, if not indeed before it had been begun, it became clear that corpuscles were not going to explain everything. Of the partial truth of the corpuscular hypothesis, of its value in explaining many previously unconnected phenomena and in inspiring fertile research, there has never been any doubt; but before we can obtain a complete representation of the structure of the atom, we must know a great deal more about the portion which is not corpuscles—the positively charged part. All that was known of this part was that it was not similar to the negative part; so much at least can be safely asserted in view of the immense difference in all phenomena in which the agents bear charges of opposite sign.

It was natural that in attacking the new problem recourse should be had in the first instance to the methods which had proved so successful in the discovery of the nature of negative electricity. Thomson (1905, 1907, 1908, 1909) once more turned his attention to the electric discharge at low pressures with the purpose of elucidating further the nature of the 'canalstrahlen,' a set of rays which travel in the opposite direction to the cathode rays, and have been proved by eminent German physicists to carry a positive charge. The previous measurements of the ratio of the charge to the mass of the particles composing the canalstrahlen, carried out by methods similar to those described on p. 164 for the cathode rays, had failed to give definite information as to the nature of the particles, on account of complications

arising from their interaction with the gas through which they passed. By repeating the measurements at the lowest attainable pressures, the use of which was rendered possible by ingenious new devices, Thomson succeeded in showing that, with one exception, whatever the nature of the gas with which the tube had been filled, the same value was obtained for the ratio of the charge to the mass of the particles. There appeared to be two groups of these particles, for one of which the ratio was 10^4, for the other 5×10^3. The former value corresponds to an atom of hydrogen carrying the electronic charge, the latter to a molecule of hydrogen bearing the same charge, or an atom of helium bearing two such charges. In only one gas, helium, could any sign be detected of particles for which the ratio was different from these values; in helium particles were found for which the ratio was 2.5×10^3, corresponding to an atom of helium bearing a single electronic charge.

In order to explain these results, it had to be supposed either that all the gases used were contaminated with hydrogen, although the greatest precautions were taken to avoid the presence of that gas, and that hydrogen and helium alone were capable of forming canalstrahlen, or that the atoms of the other gases were split up by the action of the discharge giving off portions which were very similar to the atoms of hydrogen and helium. If the latter alternative could be accepted, the existence of a positive electron, that is a positively charged particle forming a common constituent of all atoms, would be established; the positive electron, as was to be expected, would be very different from the negative, being several hundred times more massive, and probably far more complex in structure. The hypothesis of the existence of such a body had been rendered far from improbable by the observations of Rutherford, which showed that the α particle emitted by radio-active atoms during disintegration

was the same whatever the nature of the atom from which it was produced, and that the ratio of the charge to the mass of this particle was that of the second group of canalstrahlen detected by Thomson, 5×10^3. Subsequent research has shown that these a particles are atoms of helium bearing two electronic charges, and it is not possible, therefore, to suppose that they form part of the atom of hydrogen, of which the mass is less than that of the atom of helium ; but the behaviour of hydrogen is known to be so anomalous in almost all phenomena which involve ionisation, that a theory which regarded an atom of hydrogen as fundamentally different from all other atoms would not necessarily be unacceptable.

From that time until the present Thomson has continued his investigations on canalstrahlen, the positive rays of Goldstein, the magneto-cathodic rays of Villard and several other forms of rays which had been shown to be present in the discharge at low pressures. His researches, together with those of Kunz (1908) and of Wien, Stark and other eminent investigators abroad, have done much to elucidate the complex phenomena attending the ionisation of gases at low pressures, but the time has not yet come for a general review, nor is it our business here to offer an opinion concerning the many questions raised which are still the subject of controversy. It must suffice to note that Thomson considers that he has produced strong evidence for the view that the ionisation of a molecule consists in the separation from it not, as was formerly thought, of one or more negative electrons, but of a neutral doublet consisting of a negative electron and a positively charged portion, such as he found was contained in the canalstrahlen; both the positive and the negative portions would appear to be common constituents of most atoms. His conclusions in this direction were influenced by his experiments in other

directions, such as those on the electrification produced by heating salts of various kinds (1905) and by those of Wellisch to which reference has been made already (p. 228).

During the last few years an altogether new group of ideas have begun to influence experimental research. They are as yet but slightly represented in the publications from the Laboratory, but, since it is possible that they may become of great importance in the near future, it may be desirable to give some account of them.

A few years ago our knowledge of the essential features of the various forms of radiation seemed to be nearly complete. There appeared to be two totally distinct kinds of radiation (that is, of influences travelling in straight lines from the origin); one kind consisted of the α and β rays from radio-active substances, and the closely related cathode rays and canalstrahlen of a discharge tube; the second kind consisted of light and heat radiation and Röntgen rays. Opinion differed as to the nature of the γ rays, but they were usually placed in the second class. Rays of the first class were believed to consist of charged particles travelling with considerable velocities; rays of the second class were believed to be disturbances in the ether, which, in the case of light and heat radiation, had a regular undulatory form.

It might be thought that there should be no difficulty in distinguishing between agents so different in their nature, and, indeed, in respect of a considerable number of qualities the distinction between the two classes is sharp and simple. The first class of rays are deflected in electric and magnetic fields; the second class are uninfluenced. The first class of rays are also incapable of showing the phenomena of polarisation, diffraction and interference, the discovery of which in the case of light led to the final overthrow of the corpuscular

hypothesis favoured by Newton. Röntgen rays exhibit polarisation; the absence of interference was considered to be a logical consequence of the nature of the etherial disturbances of which they were constituted.

It was only when the ionisation and the secondary radiation produced by these rays were considered closely that any doubt was thrown upon the completeness of our knowledge. A great difference in respect of these processes might be expected to be shown by the two groups of radiation. If the rays consist of isolated particles, it was to be expected that in their passage through a gas only a small proportion of all the molecules in that gas would come within the sphere of their influence. But if the rays consist of disturbances in the ether spreading out continuously from the origin, like water waves from a stone thrown into a pond, it was to be expected that all the molecules would come under their influence and all be treated alike. Experiment showed, however, that the behaviour of both groups of rays was exactly similar: both groups only ionise a very small proportion of the molecules of the gas through which they pass. Again, the intensity of rays of the first class depends only on the number of particles in the rays, whereas the intensity of rays of the second class must depend on the magnitude of the etherial disturbance. Accordingly, when the rays produce secondary radiation, consisting of a stream of electrons ejected from the body which they strike, it was to be expected that a change in the intensity of the primary rays of the first class should produce no change in the velocity of the secondary rays, but only a change in their number or intensity, whereas a change in the intensity of primary rays of the second class should produce a change in the velocity of the secondary rays and, possibly, none in their intensity. Here again, however, observation showed that both types of rays behaved as rays of the first class—a change in the

intensity of the primary rays produced a change in the intensity of the secondary rays and no change in their quality.

All the work on radiation which has been mentioned previously may be considered as bearing on the questions thus raised, but the experiments of Bumstead (1906) and Innes (1907) must be mentioned as especially directed towards their elucidation. There were two obvious methods for accounting for the anomalies displayed by radiation of the second class: it might be supposed either that the views previously adopted as to the processes of ionisation and secondary radiation were at fault, or that the error lay in the accepted opinion as to the nature of the rays. Adopting the first alternative, Thomson and others suggested that the action of the rays was indirect, that the disruption of the atom in ionisation or secondary radiation was a consequence of changes proceeding spontaneously in the atom, but accelerated by the action of the rays. On this view the incidence of the rays would be analogous to the pulling of the trigger which fires a rifle: the small proportion of the atoms ionised and the independence of the quality of the secondary rays of the intensity of the primary would be explained. The hypothesis was not entirely satisfactory, because in explaining the abnormality of the rays of the second class it raised difficulties as to the normality of the action of ray of the first class. It was finally abandoned when it was shown that the nature of the secondary rays depended only on the nature of the primary and was practically independent of the nature of the atoms on which the rays acted.

It seems necessary, therefore, to adopt the second alternative and to change our views as to the nature of the second class of radiation. So far as the Röntgen rays are concerned, the solution of the difficulty may be found in the

suggestion of Bragg, that these rays should be regarded as belonging to the first class and consisting of electrically neutral particles travelling with high speeds; his able advocacy has invested the idea with much plausibility, but it is yet doubtful whether the new difficulties which it raises will not prove insuperable. However this may be, the more interesting part of the problem, the nature of light radiation, will remain: for the phenomena of interference, shown by light and not by Röntgen rays, are no less fatal to a purely 'corpuscular' theory nowadays than they were in the time of Young and Fresnel.

When his first hypothesis appeared untenable, Thomson (1907) revived a suggestion which he had put forward as early as 1903. The theory which he now set out was based explicitly on the action of ultra-violet light in setting free corpuscles from metals on which it is incident. This so-called 'photo-electric effect' had been frequently studied; among others Varley (1904) had worked at it in the Cavendish. Thomson proposed to retain the conception of light as an undulatory disturbance, but to imagine that this disturbance does not spread out from the source equally in all directions like the waves of sound in air or water, but is concentrated along a few narrow lines radiating from the source and leaving the intermediate spaces entirely unaffected. The idea may seem at first sight somewhat startling, but more mature reflection will show that it is only a logical development of the conceptions which have been applied with such great success in the molecular and electronic theories of matter: for it is the essential feature of both these theories that the actions which are apparently distributed continuously throughout the space 'occupied' by a material body are regarded as confined to an extremely small proportion of the whole volume of that space.

The theory explains at once the similarity of the action of

both classes of rays in ionisation, for the intensity of the rays would be supposed to depend on the number of lines along which disturbances were travelling and not on the magnitude of the disturbance along any one line, and, since the rays would only occupy a small portion of the space surrounding the source, they would only act on a small proportion of the atoms in that space. But it is not yet quite clear that the theory is capable of explaining satisfactorily the optical phenomena of interference and diffraction, which led to the abandonment of the corpuscular theory of light. Several researches are in progress in various parts of the world at the present time which aim at deciding this and other questions connected with the new hypothesis: and in the Laboratory some of the work of Taylor (1909) and Campbell (1909) has been directed to the same end. It is too early to express any opinion as to its final acceptance, but, since the work may lead to a novel development of physical science, it would have been undesirable to pass it by without some notice.

The remainder of the investigations pursued at the Cavendish are not easily grouped round a few leading ideas, but it must not be supposed that they are therefore less important. To those who are conversant with modern physical work the nature and results of these researches are sufficiently represented by the bibliography at the end of the volume: to render them intelligible, in accordance with the purpose of the volume, to those who are not so conversant it would be necessary to compile a complete text-book. But some of the more striking 'miscellaneous' investigations may be mentioned briefly.

G. F. C. Searle, the Senior Demonstrator at the Laboratory, to whose instruction in practical physics all students of recent years owe so much, communicated (1905, 1906,

1907) several of the more ingenious of the experiments used for teaching in his classes. But he has also found time to do much important work of purely scientific interest. He alone of all the workers in the Laboratory has maintained the old traditions of Cambridge physics, handed down from the time of Stokes, Maxwell, and Rayleigh, in publications dealing with purely mathematical physics and the practical determination of the fundamental electric and magnetic quantities. His work in the former direction (1906, 1907, 1908) has been concerned with the important theory of the motion of charged particles with high velocities, which has been treated very elaborately in recent years by continental physicists. In the latter direction his papers on 'Standards of Mutual Induction' must be mentioned (1906).

H. A. Wilson (1904) conducted with his usual experimental skill a successful demonstration of one of the fundamental theorems of modern electricity. The feature which distinguishes post-Maxwellian from pre-Maxwellian electrical theory is the hypothesis that a moving electric charge is equivalent in all respects to an electric current moving in a conducting circuit. The proof of this equivalence so far as the magnetic action of the current is concerned was given by the work of Rowland, confirmed later by Röntgen and by Pender and Crémieu. Wilson succeeded in detecting the converse of this action, the electric field produced by the motion of a charge in a magnetic field. His results were in complete accordance with Larmor's theory of dielectrics.

Two researches must also be mentioned which represented a branch of physics which has, in recent years at least, been somewhat neglected at Cambridge, the accurate determination of physical constants which have no direct theoretical significance. Horton (1905) measured with very great care the torsional rigidity constants of quartz and metal

fibres; these constants are of great practical importance, owing to the use of such fibres in suspending the moving parts of delicate electrometers and galvanometers. Hoskings (1909) measured with great elaboration the absolute value of the viscosity of water, with reference to which the viscosity of other liquids is usually determined by comparative observations.

Attention must also be directed to E. F. Burton's work (1906) on colloidal solutions of metals, those curious mixtures which seem intermediate between true solutions and mere suspensions of fine particles, and appear likely to throw much light on the mechanism of solution; also to Barlow's (1905, 1906) and Vegard's (1908, 1909) work in an allied field, osmosis, a phenomenon which the continued efforts of physicists for thirty years have done little to elucidate.

The idea seems to be widely current that the Cavendish Laboratory is a place of narrow specialisation, that all the researches carried out in it are directed to the solution of a small branch of physical problems, which have little connexion with the general body of the science. And this chapter may seem to have given some support to this idea by devoting so much space to some investigations and giving bare mention to a much larger number. But this view of the work of the Laboratory is absolutely false. It is quite true that there, as anywhere else where much valuable research is done, the ideas involved are largely those which are of special interest to one distinguished leader. But these ideas do not represent to-day a mere offshoot of physics: they lie in the direct line of progress of the science. Nor are they the only ideas which lead to investigations of the highest importance, and it must not be supposed that the space devoted in this chapter to any piece of work is a measure of its value or its interest. For those

who are following closely the evolution of our study the entries in the list of memoirs published is a sufficient indication of the contributions of the Cavendish Laboratory, but the space at our disposal necessitates that the treatment should appear somewhat one-sided to those whose acquaintance with the subject is less profound.

CHAPTER IX

The Development of the Teaching of Physics in Cambridge

In October 1871 Professor Clerk Maxwell delivered an introductory lecture on Experimental Physics in the University of Cambridge to inaugurate his tenure of the Chair in that subject, which had just been founded.

The first part of the lecture dealt with the functions of the new Physical Laboratory shortly to be erected, and laid down some of the general principles which should govern the teaching of Experimental Physics. He pointed out that the experiments which might be made could be divided into two classes, experiments of illustration and experiments of research, differing with respect to the motive which actuated the experimenter. In the former class the experiments are presented to the student in order to help him to a more vivid and accurate realisation of the principles of Physics, whereas in the latter the student uses his knowledge of these principles as a guide, while he devises and performs a series of experiments, whose object is to question and, when necessary, to cross-examine Nature with regard to the matter which is being investigated. The former class of experiment has its natural home in the lecture room and the latter in the laboratory, as was pointed out by Maxwell, but at the same time he clearly indicated that the teacher should not

only exhibit experiments of illustration himself, but should also encourage his students to make them.

Later on in the lecture Maxwell touched upon the class of man who might be expected to take up the study of Experimental Physics in the University, and one can trace in his remarks a suspicion, which afterwards proved to be well founded, that few undergraduates, while still preparing for the Mathematical Tripos, could be induced during that time to add to their work any study which did not directly tend to improve their place among the Wranglers, although it is especially to minds well trained in mathematics that the pursuit of practical science promises interest and fruitfulness.

In one of his letters written at this time Maxwell playfully suggested that the desideratum at the new Laboratory was to set a Don and a Freshman to observe and register together, the latter to serve as a check upon the former, but it was not found practicable to effect this union between the lion and the lamb. When the Laboratory was opened a goodly company of Dons, most of whom had taken high mathematical honours, was soon at work, but the supply of undergraduates could not be expected to be large, for the total number of names in the Natural Sciences Tripos in 1873, the year when Maxwell first examined, was only 19.

After Maxwell's death in 1879, and the election of Lord Rayleigh to the Cavendish Chair, came the retirement of W. Garnett, who had held the Demonstratorship of Physics during Maxwell's tenure and had given him assistance and help which he greatly valued, and R. T. Glazebrook and W. N. Shaw began their long period of service in the Laboratory as Demonstrators. They had the help of J. H. Randell of Pembroke and J. C. McConnel of Clare as Assistant Demonstrators, and the course of instruction in Practical Physics for

undergraduate students was gradually extended and systematised. The classes grew larger, and it began to be found necessary to supplement individual instruction by MSS. describing the practical details of the various experiments. The written notes thus gradually compiled became even more indispensable as time went on, and finally in 1884 they were recast, amplified by some chapters dealing with general principles of physical measurements and calculations, and published, forming the well-known 'Glazebrook and Shaw's Practical Physics,' which has made so lasting an impression upon the teaching of the subject in this country.

In 1881 a change in the University Examinations came into operation which at once produced a very great effect upon the teaching of Physics. The Natural Sciences Tripos was divided into two parts; in Part I. candidates were expected to take the more elementary portions of three or more subjects, while in Part II. they were encouraged to specialise in a single subject, though a competent knowledge of a second subject was also required if a first class was to be obtained. Part I. could be taken at the end of the second year of residence, and Part II. at the end of the fourth year, so that candidates had the opportunity of working continuously for two years at the more advanced parts of a selected subject without the distraction of having to maintain their knowledge of a considerable number of other subjects at examination pitch. The Regulations were further altered so as to allow a candidate who had taken Honours in Mathematics to present himself for Part II. of the Natural Sciences Tripos without passing Part I., a relaxation which it was hoped would be of especial value to men who contemplated the serious study of Advanced Physics.

Before these changes were made, the number of

candidates taking the Natural Sciences Tripos in each year had settled down to an approximately steady state, oscillating between 25 and 30. In 1883–4, when the initial disturbances caused by the transference from one system to another had subsided, 35 candidates took Part I. and 18 took Part II. After this, while the number of candidates for Part II. remained practically unaltered, Part I. showed a continuous and rapid increase. In 1900 the number was 122, and the process of growth is still continuing.

Although, as we have seen, the changes in Regulations did not lead to a large increase in the number of students of Advanced Physics, they made a fuller and more thorough study of the subject possible, and, as a result, the Laboratory Courses for such students were greatly developed in the next few years, and schedules of Advanced Demonstrations in Properties of Matter, in Heat, in Optics and Sound, and in Electricity and Magnetism were drawn up. The standard and general nature of the work done in these courses can be inferred from a little book describing some of the experiments on Heat, which was compiled by Shaw in 1885, and published a year later.

It was a notable feature of these Advanced Classes that practically nothing in the way of description of the experiments or explanation of their theory or instruction as to their details was written out by the Demonstrators. Instead of this, they selected a representative set of more or less advanced experiments in the various groups into which the subject was divided, and in each case supplied references to text-books and original papers, which enabled the student to collect and arrange for himself the preliminary knowledge requisite for the attack of the problem proposed to him.

The organisation of the Advanced Demonstrations differed from that of the Elementary Classes in another detail, which, though apparently of small importance, was

in reality very significant. In the Elementary Classes it was soon found that the most satisfactory plan for training students in the art of taking notes was to appropriate to each person a separate MS. book in which he was required to enter the measurements and the results for each experiment. In this way the work of each student was kept together, and it was made easier for the Demonstrator to see the progress that was being made and the points upon which advice or help were needed. In the Advanced Demonstrations, on the other hand, an MS. book was set aside for each experiment, and the student entered in it an account of the methods which he had employed and the results which he had obtained. These records remained the property of the Laboratory; the better students took a very natural pride in writing them up fully, and they have furnished valuable guidance and most interesting reading to many generations of their successors, who have often been able, in their turn, to suggest further improvements and refinements.

It was not considered to be either possible or desirable that a student should perform all the experiments included in the courses. On the contrary, he was encouraged to give a good deal of time and thought to each experiment which he undertook, and to gain a complete mastery of it, learning to perform the manipulations involved, determining the various sources of irregularity and error, investigating the necessary corrections, and estimating the degree of accuracy which it was possible to obtain. It was no uncommon thing for a student to devote several weeks to a single experiment, working with the utmost interest and keenness throughout. It is hardly necessary to insist upon the value of the mental training afforded in such a case.

In 1884 Lord Rayleigh resigned the Professorship

of Physics, and Professor J. J. Thomson's tenure of office began. Then, in 1887, a University Lectureship in Experimental Physics was created, and Shaw retired from his demonstratorship on his appointment to the new post. This, however, was a change of title rather than of function, and, although he was no longer personally responsible for directing it, he still remained in complete touch with the practical work done in the Laboratory.

H. F. Newall was appointed to the vacant demonstratorship and took charge of the Elementary Classes. At the same time I had the good fortune to be appointed to an assistant-demonstratorship, in which capacity I found myself under Newall as my immediate chief.

Though my present purpose is to give an account of the teaching rather than of the teachers, I owe so great a personal debt to Newall that it would not be proper for me to pass it over in absolute silence. It was impossible to work under him and see not only his ability as a physicist and an organiser, but also his unfailing patience, kindness, tact and good-humour to his subordinates and his students, without deriving instruction and inspiration from his example. During all my time at the Cavendish Laboratory I had nothing but consideration, kindness and help alike from my Professor and my colleagues, and I look back to it with the utmost gratitude, but it fell to Newall to take me in hand when I was absolutely raw and untrained, and I may therefore be pardoned a few words of especial acknowledgment to him.

In 1890 another re-arrangement of the posts in connexion with the Laboratory was made. Glazebrook was desirous of being relieved of the work of personally supervising the students in the Advanced Courses, for which he had hitherto been responsible, and in consequence a new post, that of Assistant Director of the Cavendish Laboratory,

was created for him, and he retired from his demonstratorship, though he continued to give general superintendence and help in the Laboratory and to deliver some of the Advanced Lectures.

At the same time the Laboratory lost the services of Newall, who retired in order to undertake another branch of scientific work. A large refracting telescope, which had been the property of his father, had just been presented to the University, and Newall consented to become responsible for the performance and superintendence of the researches which the acquisition of this fine instrument rendered possible.

In filling up the vacancies thus created, the distribution of duties among the staff was slightly altered. The number of demonstratorships was increased from two to three, and to these posts G. F. C. Searle, S. Skinner and Wilberforce were appointed. Searle took the control of the Elementary Classes, having T. G. Bedford working with him as Assistant Demonstrator, an arrangement which has continued up to the present time; while Skinner and Wilberforce took joint charge of the Advanced Classes, and had, in addition, work in connexion with two newly-introduced sets of classes, which will be described presently.

While Glazebrook and Shaw were Demonstrators they divided between them a course of lectures in Advanced Physics of the standard of Part II. of the Natural Sciences Tripos. In addition Glazebrook gave an elementary course which met the needs of candidates for the First M.B. examination and beginners in Physics who proposed to take Part I. of the Tripos, while Shaw gave a rather more advanced course, suitable for the better candidates for Part I., in which he aimed not so much at covering ground wider than that traversed in the elementary course as at putting principles and results which ought to

be already known to his hearers into a new light, and enabling students to detect and overcome the various difficulties which have of necessity to be slurred over when any science is being studied for the first time.

Professor J. J. Thomson, continuing and extending the practice of Lord Rayleigh, delivered annually two courses of lectures, one on Properties of Matter and one on Electricity and Magnetism, which were not formally arranged in connexion with the standard of either part of the Tripos, but which were most stimulating and interesting, and always attracted a large class, not only including all the workers at the Laboratory whose heart was in their subject, but also containing a considerable number of students in other branches of learning who were attracted by his personality.

When Glazebrook retired from his demonstratorship he also gave up some of his teaching work in connexion with Trinity College, and J. W. Capstick was appointed by the College to the lectureship in Physics thus vacated. To the mutual advantage of the College and the Laboratory it was agreed that Capstick's duties should include the delivery of the Elementary Lectures described above, and this arrangement continued in force for eight years.

Meanwhile a new group of classes in Practical Physics had been developed at the Laboratory in connexion with these Elementary Lectures. Though, as has been stated, the lectures were attended by some Tripos men, the candidates for the First M.B. Examination, who had been steadily increasing in number for some years, had come to form a large majority of the class. In the First M.B. there were two papers in Elementary Physics, including Mechanics, but no oral or practical examination in these subjects was held, and doubts began to be felt as to the adequacy of this test and the value of the knowledge likely to be acquired by candidates who were preparing for it.

About the year 1887 these doubts were set permanently at rest by the institution of a simple oral examination in physical apparatus, and the memories of the first examiners almost collapsed under the strain of storing for future recital the immense collection of 'howlers' perpetrated by their victims. Then we heard for the first time of the man who recognised in a thermometer a machine for determining the specific gravity of water, although, when he was furnished with the thermometer, a basin of water and a bit of string, he failed to achieve any numerical result. Again, there was the man who said that a compass needle mounted over a graduated circle was an instrument for determining the latitude and longitude. 'What,' said the examiner, aghast, 'can you determine the latitude and longitude with this?' 'No, sir,' said the man, 'but *you* can, sir,' a trustfulness which deserved a higher reward than it received. It is only fair to give, on the other side, an instance in which sheer common sense brought its possessor scatheless through his ordeal. A spirit level was given him, and he was asked which was the higher end of the examiner's table. He put the level down, and duly pointed in the direction of the bubble, but the tube of this level was not parallel to the base, and the examiner turned the level round, saying 'And which end is higher now?' The candidate realised that the bubble had moved and that the table, apparently, had not. He pondered deeply, then suddenly illumination came; the noble rage of the scientific discoverer lifted him above the petty restrictions of propriety as completely as if he had been Archimedes, and he cried out triumphantly, 'Why, the bally thing's cock-eye!'

Very soon after the year 1887 the University of London instituted a simple practical examination in addition to the papers in Physics hitherto set in the Preliminary Scientific Examination for the Degree of M.B. and the Intermediate

AN ELEMENTARY CLASS

Examination in Science for the Degree of B.Sc. The earlier examiners, of whom I was one, were faced by a mass of absurdity even greater than that which had been revealed at Cambridge, and the most disquieting part of the matter was that very often candidates who seemed from their written work to have a real knowledge of their subject made the grossest possible blunders when confronted with the actual apparatus of which they were able to furnish perfect diagrams and descriptions on paper.

In order to make good the deficiencies which had been brought to light at Cambridge, Professor Thomson in the year 1887 appointed T. C. Fitzpatrick to an assistant demonstratorship and gave him the duty of organising a class in elementary practical physics for medical students in connexion with Glazebrook's lectures. It could not be expected that the experimental work accomplished by such students in the limited time at their disposal could do much towards training their powers of investigation and manipulation; but, to return to Maxwell's classification, while experiments of research were impracticable, it would be of great help to the students if they were set to perform for themselves suitable experiments of illustration, supplementary to the lecture experiments of which they had hitherto been somewhat passive spectators.

The new practical classes were not at first too large to be taken by one man. They were held in the lecture-room, as no other place was available; owing to the very small experimental skill possessed by the students it was necessary to design apparatus which should be what is unkindly called 'fool-proof,' and Fitzpatrick's colleagues will always remember how they used to peep into the room when a class was in progress, and see him behind the lecture-table presiding over a huge steaming caldron from which he dealt out over the counter to his students large flasks of water

fitted with corks and tubes which were being used for the determination of coefficients of expansion.

In a very short time the classes grew larger, and when they had been in existence for about a year Professor Thomson associated me with Fitzpatrick in their management. In our early days we had all kinds of difficulties to surmount in addition to those directly connected with teaching. The new classes had somehow to be fitted into a laboratory already over-full, the exigencies of the available space and apparatus demanded that the men should be divided up into small groups, then times had to be found at which each group could attend, and these times were dotted about all over the day and extended so late into the night that the porter at the gate of the New Museums finally sent in a formal complaint that he was being deprived of his covenanted hours of sleep. At this juncture, in 1890, a corrugated-iron shed, which had been used for some time as a dissecting-room by the Department of Human Anatomy, fell vacant, and we considered ourselves fortunate beyond measure when we were told that we might take possession of it for the Medical Students' Practical Physics Class. It was true that even here some crumpled rose-leaves disturbed our repose of spirit. The elimination of the odour which had been left by our predecessors in the room was not accomplished till Fitzpatrick and I took the question up personally. Washing with carbolic acid, chloride of lime, etc., seemed at first effectual, but when the smell of the disinfectants cleared away the original offence returned. Finally, having closed all the windows, we put a jam-pot containing salt and manganese dioxide on each table, ran round with bottles of sulphuric acid, pouring some into each pot, escaped the liberated chlorine, and then closed the room for a couple of days. After that drastic treatment it was fit for occupation.

The only other difficulty which we had in connexion with the earlier occupants of our new abode was with the inexpensive but very small boy who was allotted to us as our laboratory assistant. We could get no work out of him between the classes, for his fear of ghosts gave him leisure for nothing but crying whenever he was left alone in the room, and he had finally to be handed over to the Professor to be reasoned with.

However, when these initial difficulties were surmounted, we found the new room very well adapted for our purpose. It was large enough to permit us to take a considerable number of men at once, thus making it possible to construct a satisfactory time-table. The enlarged classes required more than two demonstrators if the teaching was to be satisfactory, and from that time forward we had a series of men working with us as additional demonstrators, who gave us very valuable assistance not only in conducting the classes, but also in planning out the experiments and devising and making the necessary apparatus. A complete list cannot be given, but in particular we received help from R. S. Clay, W. A. D. Rudge, and C. T. R. Wilson which ought to be specially acknowledged.

The plan upon which we worked was to arrange for each week a group of simple experiments illustrating that part of the subject which had just been dealt with in the lectures, and to get together a supply of apparatus sufficient to enable each student, who only came once a week, to go through all the experiments in the group.

At first we found that so much of the time of the demonstrators was taken up in merely pointing out the experiments to be done and setting the men to work, that there was little opportunity for rendering the teaching effective by giving help and criticism and asking questions while the experiments were in progress. To get over this disadvantage, a short

account of the various experiments to be performed was drawn up, in which the apparatus and the way of using it were described in sufficient detail to enable the men to start work for themselves, and thus the demonstrators were set free for their more important duties.

We soon came to see that a great deal of the work which we were directing produced a very small impression upon the men who performed it on account of their lack of power to take suitable notes recording their observations, and after struggling with this difficulty for some time we were forced, almost against our will, to issue to our classes not only descriptions of the experiments to be done but also forms showing the proper way in which the results should be entered. We found these descriptions and instructions so useful to our own students that we thought they might be of service to others in a similar position, and so in 1896 we published our 'Laboratory Note-Book of Elementary Practical Physics,' which embodies them. Our experience, which is borne out by that of other teachers, is that, if a student wants an excuse for performing his work mechanically and unintelligently, such a book may indeed supply it, but to men who are beginners and want to make the best use of their opportunities it is a very real help, and if large classes are to be taken by any reasonable number of demonstrators it is almost a necessity.

While the developments just described were in progress, the need for some extension of the Cavendish Laboratory was gradually growing more urgent. At last it was partially met, and the new buildings which were opened in 1894 contained a very fine room into which the Medical Classes were moved, and which provided ample accommodation for the further increase in size of the individual classes which had begun to be desirable.

The rapid increase in the number of candidates for Part I.

of the Natural Sciences Tripos has already been described, and, as the proportion of students taking Physics was well maintained, the Elementary Class grew inconveniently full. Some change of arrangement became desirable, and the pressure was relieved by the introduction of a new Practical Class in connexion with Shaw's lectures. This class, at first conducted jointly by its inventors, W. N. Shaw and H. F. Newall, was put on the retirement of the latter into the charge of Skinner, and differed in some important details of organisation from the Elementary Class which Searle was at that time developing along the lines originally laid down by Glazebrook and Shaw. In the latter class it was only found possible to have one example of each piece of apparatus, and so the order in which the experiments were done by the students was not wholly under control, but had to depend upon the apparatus which was at liberty. The demonstrators of course did their best to keep this order from being too illogical and to guard against any assignment to a student of an experiment the theory of which he had not yet reached in his reading or lectures.

The plan adopted in Skinner's class was to include a rather smaller number of experiments, for each of which several specimens of the necessary apparatus were obtained. The experiments were devised so as to be in direct connexion with the lectures, and, by a suitable system of grouping, the experimental work of each student was kept in close contact with that part of the subject which had recently been discussed by the lecturer. The experiments, like the lectures, were designed for men who already had a fair elementary knowledge of Physics and aimed at the deepening rather than the widening of this knowledge.

It will be seen that the scheme of teaching resembled to a certain extent that of the Medical Classes. In the latter,

however, the simplicity and inexpensiveness of the apparatus made it possible to have such a supply that all the students could be doing the same experiments at the same time, an innovation beyond the wildest dreams of earlier teachers of Physics. The apparatus in Skinner's class could not be multiplied on the same lavish scale, and so a great deal of organising capacity was called for in order that the experiments and the lectures should be kept satisfactorily together.

While these new Practical Classes were being developed, the work of those which already existed was being consolidated and extended. In the Advanced Class the schedules of the various courses were revised from time to time, as alterations and additions were suggested by the experience which we had obtained, and, in particular, Skinner drew up a very interesting course of experiments in Chemical Physics.

In the Elementary Class, when it came in the year 1890 under the direction of Searle, who had worked in it for the two preceding years as an Assistant Demonstrator under Newall, progress at first was rather of the nature of revision and extension than of change. Indeed, so well had the general scheme of that class been thought out in the first instance, that when a course of experimental work of a somewhat different character was introduced into it later, it was as an addition to, and not a substitution for the work which hitherto held the field.

For the first three years the development of the teaching in the class was such as could be naturally expressed in a new edition of Glazebrook and Shaw's 'Practical Physics.' The test of time and use had indicated passages where revision was desirable; some new experiments in harmony with the general plan of the book were introduced, and in 1893 the authors brought out a second edition, in the

introduction to which the influence of Searle was suitably acknowledged.

As time went on, however, and the students attending the class not only grew larger in number, but were found to include a proportion of men who had already made fair progress in the subject and would be attracted by experiments somewhat more heroic than those included in the ordinary course, Searle continually devised new pieces of work, some of the utmost ingenuity, and wrote up manuscript accounts explaining with the greatest care and fullness the experimental details, as well as the physical principles and the mathematical calculations involved. Many of these experiments first saw the light as problems set for the better candidates in Physics for Part I. of the Natural Science Tripos. Some of them appeared when Searle was an examiner, many more when he was not, for he showed the greatest generosity in putting his ideas at the service of others, and it soon became the recognised practice for the examiners to have the help of Searle in the setting of questions for the practical examination as well as in making the arrangements for conducting it.

The general idea underlying these experiments was that they should illustrate points of theory, particularly of mathematical theory, somewhat beyond those of the ordinary course, that they should be susceptible of a high order of numerical accuracy, and that they should be performed with carefully designed apparatus, in which even the smallest details generally contained some points of interest, and proper attention was given to the geometry of the necessary movements and constraints.

Some of the apparatus so designed is now accessible to all teachers of Physics, as it appears in the lists of various instrument-makers, and it should have a far-reaching influence for good upon the present methods of construction

of a good deal of the equipment of a laboratory for students in Physics.

It is hoped that in a short time much of the work to which Searle has given himself with such devotion will be published; his book on Experimental Elasticity, issued in 1908, forms a first instalment, and a similar treatise on Optics is expected soon to follow it.

Another extension of the work of the Elementary Class had been introduced soon after 1886. It has been already pointed out that when the new Regulations for the Natural Sciences Tripos came into operation it was hoped that more men who had taken the whole or a part of the Mathematical Tripos would find themselves able to pass to the study of Physics. Although these hopes were hardly realised, so far as the Advanced Physics of Part II. of the Tripos was concerned, an increasing number of men after having passed the earlier part of the Mathematical Tripos proceeded to take Physics for Part I. of the Natural Sciences Tripos, most of whom expected that their future career would be that of a teacher. A considerable proportion of these students, though of course well able to read for themselves the theoretical portion of their new subject, had little or no knowledge of experimental methods, and so, in order that they might start the work of the Michaelmas term in an adequate state of preparation, a special session of the Elementary Class was held for them in the Long Vacation, to which no other students were admitted unless by express permission of the Professor. By restricting the numbers of the class in this way it was more feasible to allot experiments in an order suited to the needs of such students, and a great many of them made very rapid and satisfactory progress.

In 1896 a new development took place in the Laboratory, which, though not directly connected with the teaching in the most restricted sense of that word, soon came to exercise

a considerable influence upon it in various ways. Regulations came into operation which dealt with Advanced Courses of Study and Research, and prescribed the conditions under which Advanced Students from outside were admissible to these courses. The history of this movement is treated elsewhere, but some of its effects come within the scope of this chapter.

First of all, in the case of the Advanced Students from other seats of learning, over whom Professor Thomson exercised direct supervision from the time when they first appeared at the Laboratory, we had an example of the process of teaching by research.

As time went on, and the number of Advanced Students steadily grew, it became apparent that, while many brought much knowledge and skill to their work from the first, a certain proportion were not at all above the standard of our candidates for Part II. of the Tripos, and we had the opportunity of comparing the results of research teaching on such men with those which could be obtained by the methods of our Advanced Courses.

There is no doubt that much may be said on both sides. A man who works systematically through a suitably selected course gets some knowledge of all the various branches of Physics and their relation to each other, and his intellectual armoury will be well supplied with the various weapons and appliances necessary for his future campaign; while a man who takes up too early a piece of research work, it may be of very limited scope, is in danger of developing a kind of mental short-sightedness by his exclusive devotion to one particular group of physical phenomena, his range is unduly narrow, and even the work upon which he is so closely engaged may suffer from his want of familiarity with other departments of Physics, which, though apparently unrelated to what he is trying to do, would have been able to give him

much illumination and assistance. The ideal investigator is the man who not only sees Physics steadily but sees it whole.

But, on the other hand, a man undoubtedly has far more keenness and interest in his work if the knowledge of which he is in pursuit is new to the rest of the world as well as to himself. The progress of our own Advanced Courses furnished on a smaller scale a striking example of this. In the earlier days of these Courses, before the various mischances liable to occur during each experiment had shown themselves, we were not always able at once to diagnose the malady when anything went wrong, but we made suggestions when necessary, and finally the source of the trouble was traced. Later on, however, when all the symptoms had become familiar to us, we could detect a distinct slackening of interest on the part of the student when anything went amiss. Even though we tried to disguise our knowledge the men discovered that we had it, and they could not be induced to work with the same vigour to find out for themselves something which they felt that their teachers might tell them at once if they pleased. It may sound paradoxical, but it is the result of our experience that the more advanced students work best when their demonstrators appear to be in difficulties, and so from time to time we devised new experiments in order to afford our men such stimulus as could be derived from this spectacle.

In a recent address to the British Association Professor Thomson has laid great stress upon the importance of maintaining the enthusiasm of a student in his work. He states very emphatically that he has found want of enthusiasm to be a much more frequent cause of failure than either want of ability or want of knowledge among the men who have conducted research under him, and he warns all who are working for the advancement of science against the damping

effect produced upon enthusiasm by an unduly prolonged and intensive course of academic study of a single subject.

Again, a sustained piece of original work affords in many respects a better mental training than a series of detached exercises. The qualities of imagination and resourcefulness are kept more fully employed by the various difficulties which arise, because they more often come from unexpected directions, while the powers of observation and reasoning are called upon to an extent which is at least equally great.

Finally, in the case of a man who devotes himself too long to the academic study of a subject there is always the danger that he will come to regard his mental equipment as an end in itself, and that his pleasure in his intellectual weapons will lead him to be so continually polishing and sharpening them that he will be left with neither energy nor time to use them in the great struggle by which the frontiers of knowledge are enlarged.

However, although the psychological moment in the career of a student at which it is advisable to replace systematic exercises by original research may remain a matter for discussion, there is no doubt that the extension of the new method of teaching still further increased the vitality of the Laboratory. Even to men who were still following the ordinary routine for the Tripos, the fact that official recognition could afterwards be obtained for post-graduate work of sufficient merit by the award of a Certificate of Research was of service in defining and sustaining their interest in their present course of study; and the natural pride taken in the Cavendish Laboratory by all its workers was still further increased by the unrivalled magnitude of the scientific output which now came from it.

Of course, the addition of a large number of Advanced

Students to the already teeming population of the Laboratory brought with it some minor inconveniences. At times the crowding was excessive and justified the friendly critic who stated that at the Cavendish Laboratory there was more Physics done to the square centimetre than at any other place in the world. Great relief was felt when the opening of the new buildings in 1908 permitted a change in the unit of area. Limited space also contributed to a chronic shortage not only of apparatus but of all portable appliances, such as retort-stands and indiarubber tubing. For these various necessaries the competition both among the Research Students and between them and the Advanced Classes waxed very keen, and it was occasionally conducted by raids which forced one victim to describe himself as pursuing his investigations with his apparatus in one hand and a drawn sword in the other.

Another way in which the presence of so many new workers rendered great service to the teaching in the Laboratory was that it provided a source from which additional demonstrators for the various classes could be drawn whenever the necessity arose, and thus gave very material assistance in the labour of administration.

In connexion with the influence of research upon teaching, some mention should be made of the work of the Cavendish Laboratory Physical Society, founded by Professor Thomson in 1893, which met once a fortnight at the Laboratory during the Michaelmas and Lent Terms. At each meeting of this Society several short papers were read giving an account of some recent piece of research, and the papers were followed by discussions in which a great deal of interest was generally maintained. The proceedings of the Society were not very formal, and it published no Journal, but it served a most useful purpose by building up in the students an interest in the advances which were

being made in their subject, and by enabling them not only to follow the details of an investigation but also to partake in and profit by a frankly critical discussion of its methods and conclusions.

When the Advanced Students had taken their place in the work of the Laboratory, the Physical Society naturally grew even more vigorous. Not only was a greater variety of speakers assured, but the papers under discussion could more often include researches carried on in the Laboratory and either just completed or still in progress, and thus the various topics assumed a personal as well as a scientific interest.

When the teaching of practical physics at Cambridge had experienced in its various branches the developments which have now been described, there came a time during which, while the work of the existing courses received the benefits of accumulated experience, the introduction of a new course was comparatively infrequent. In fact, as to this part of its work, the subsequent history of the Laboratory has been marked by changes of men rather than of methods.

In 1898 Glazebrook left Cambridge on his appointment as Principal of University College, Liverpool, and Shaw, while retaining his University Lectureship, succeeded him as Assistant Director of the Laboratory. In the same year Capstick became Junior Bursar of Trinity College, and thus could not continue to deliver the elementary lectures, which were in consequence committed to the charge of Wilberforce and Fitzpatrick. In 1900 Shaw left Cambridge for London to take up the post of secretary of the Meteorological Council, and a redistribution of the duties which he resigned had to be made. It was thereupon decided to abolish the post of Assistant Director of the Cavendish Laboratory and to create in its stead a second University

Lectureship. To the two lectureships thus rendered available Searle and Wilberforce were appointed, but before the latter had the opportunity of delivering a single lecture in his new capacity he was called away from Cambridge to take up the Professorship of Physics at University College, Liverpool; C. T. R. Wilson thereupon succeeded him as University Lecturer and as demonstrator, while Fitzpatrick took over the full course of Elementary Lectures. The course of lectures for the Natural Sciences Tripos, Part I., which had been delivered by Shaw since its commencement, was put under the charge of Skinner.

In 1904 Skinner was appointed to the principalship of the South-Western Polytechnic and left Cambridge for London, while P. V. Bevan succeeded him at the Laboratory. In 1908 Bevan, in his turn, left Cambridge on his appointment to the chair of Physics at Holloway College, and A. Wood thereupon took his place.

The changes which have occurred in the teaching staff of the Laboratory since its opening are necessarily so numerous that they cannot readily be followed unless they are exhibited in a tabular form. A classified list has therefore been compiled and is given in an appendix to this chapter.

Although, as has already been pointed out, the changes which have been made in the practical classes during the past ten years have been but slight, the courses of lectures have steadily continued to grow both in number and in variety up to the present time. It would occupy too much space to follow up in detail all the courses which have been delivered, even during the twenty-five years which this book commemorates, but a list of the lectures given in a few selected years will show the enormous development which has taken place in this branch of the teaching of Physics.

In 1880 the only lectures announced were a course by Lord Rayleigh, one on radiation by Schuster, and one by Stokes, while Glazebrook and Shaw were responsible for the demonstrations; but in the next year the list of lecturers was enlarged by the addition of the names of Glazebrook, Shaw, Garnett, and Coutts Trotter. In 1885 the lectures available for students of Physics were a course on hydrodynamics and optics by Stokes, a one-term course on properties of matter and electrostatics by J. J. Thomson, the advanced course by Glazebrook and Shaw, some advanced lectures by Hart at St. John's College, the course for Part I. of the Natural Sciences Tripos by Shaw, the elementary course by Glazebrook, and a course for the first M.B. examination by Atkinson at Trinity Hall.

In 1890 the three-term course on properties of matter and electricity by Thomson, to which allusion has already been made in this chapter, had been in existence for some years. Besides this, Thomson was giving a course in mathematics for students of Physics, and one on the kinetic theory of gases, and was also setting, in collaboration with Glazebrook, a series of papers for candidates for Part II. of the Natural Sciences Tripos. In other respects the arrangements were the same as in 1885, except that Wilberforce was delivering lectures for the first M.B. examination at Caius College, and had been giving in 1888 and 1889 a course of lectures and practical work on dynamo machines. In 1890 Ewing was elected to the chair of Mechanism and Applied Science at Cambridge, and the need for such a course at the Cavendish Laboratory naturally disappeared.

The changes to be noticed in 1895 were that Thomson was lecturing on the electromagnetic theory instead of on the kinetic theory of gases, the papers for Part II. candidates were no longer set, and Atkinson and Hart had left

Cambridge. Capstick had taken over the Elementary Course from Glazebrook and was giving in addition two one-term courses on sound, one suitable for Part I. and the other for Part II. of the Natural Sciences Tripos. Whetham was lecturing at the Laboratory on solution and electrolysis and on chemical physics, and Skinner was giving a revision course for the first M.B. examination. A course for the same examination was also being given by E. H. Griffiths.

In 1900 Thomson had handed over the lectures on mathematics for students of Physics to Townsend and had substituted two courses, one on electric waves and one on the discharge of electricity through gases, for the single course on electromagnetic theory which he was delivering five years previously. Searle and C. T. R. Wilson, as University Lecturers, were giving advanced courses, the one on electrical measurements and heat, the other on light. Advanced courses were also being given by Whetham on solution and electrolysis and by Capstick on sound. Skinner was delivering at the Laboratory a course for Part I. of the Natural Sciences Tripos in succession to Shaw, and Whetham a course of similar scope at Trinity College. Fitzpatrick was in charge of the Elementary Lectures at the Laboratory and was giving a revision course of the same standard in addition. The course by Griffiths for the first M.B. examination still continued to be given, and revision courses for the same examination were being conducted by Whetham at Trinity and by D'Arcy at Caius. Shaw, although he had ceased to be resident in Cambridge, was prevailed upon to return from time to time in that year in order to deliver a course on the physics of the atmosphere.

In 1903 we had to mourn the death of Sir G. G. Stokes. The many generations of students who have attended his

lectures on optics will never forget the feelings of affectionate veneration which his personality inspired, or the hours during which they watched him while he repeated, with apparatus of archaic simplicity, and with an enthusiasm which could not fail to be contagious, the classical experiments of a department of Physics which owed so much to his investigations.

In 1905 Larmor, who had succeeded to the Lucasian Chair of Mathematics, was taking as his subjects electrodynamics and thermodynamics. Thomson was lecturing upon the corpuscular theory of matter instead of upon electric waves. Whetham's advanced courses had been changed so as to include the thermodynamics of Physics and Chemistry and the theory of electrolytic dissociation. Capstick was no longer lecturing, but Wood was giving a new course at the Laboratory on heat and sound for Part I. of the Tripos. Bevan had succeeded to the lectures on mathematics for students of Physics and to Skinner's course for Part I., and Griffiths had left Cambridge to take up the position of Principal of the University College of South Wales in Cardiff. In other respects the lecture list resembled that of 1900.

Finally, in 1910, the list promulgated by the Special Board for Physics and Chemistry includes the following lectures in Physics: Larmor, electricity and magnetism, electrodynamic and optical theory, and thermodynamics and theory of gases; Thomson (for Part II. of the Tripos), some relations between ether, matter and electricity, discharge of electricity through gases, and (for Parts I. and II.) properties of matter, and electricity and magnetism; Searle (for Part II.), heat, and electrical and magnetic measurements; C. T. R. Wilson (for Part II.), light; Whetham (for Part II.), thermodynamics of Physics and Chemistry, theory of electrolytic dissociation, and (for

Part I.) heat and properties of matter, and electricity and magnetism; Wood (for Part I.), mechanics and properties of matter, light, and sound; Spens (for Part I.), mechanics, properties of matter, and revision papers; N. R. Campbell, principles of physical science; Gold, meteorology; Fitzpatrick (for first M.B.), mechanics and hydrostatics, heat and light, and electricity and magnetism; Horton (for first M.B.), revision course in heat, light and electricity; D'Arcy (for first M.B.), revision course in heat, light and electricity, and revision course in mechanics and hydrostatics. The practical courses announced in the official list include the advanced demonstrations by Wilson, the elementary demonstrations by Searle and Bedford, a new course of elementary demonstrations by Spens for Searle, the course of demonstrations by Wood in connection with his lectures, the ordinary three-term course of demonstrations for medical students, and two one-term revision courses of the same character by Horton for Fitzpatrick. In addition to these, Searle and Bedford are delivering, under the auspices of the Special Board for Mathematics, a course of experimental lectures in geometrical optics for Part I. of the Mathematical Tripos, a new departure which has already met with a considerable measure of success.

When this list is carefully considered many points of interest are revealed. In the first place, the very large volume of first-rate work which it represents is obvious to every competent judge, and no one who has the welfare of the Cavendish Laboratory at heart can fail to experience a thrill of pride when he reflects upon it. Further, it will be seen that candidates for Part II. of the Natural Sciences Tripos are now exceptionally well provided with lecture courses on the various departments of advanced Physics sufficiently detailed and substantial to permit of that degree

of specialisation which the growth of the subject has rendered inevitable. The foundation of two University Lectureships in Physics, to which teaching of this character is attached as a primary duty, has done much to assist in this development, and, although it will be noticed that there are still a few gaps which might well be filled, it cannot be denied that the solution of a difficult problem in organisation is being reached in a very satisfactory manner.

When the courses for Part I. of the Tripos are considered, it will be noticed that administrative difficulties of another kind have now been overcome. The rapid and continuous growth in the number of candidates taking Physics in this part of the Tripos has already been mentioned, and the earlier measures which were taken to diminish the pressure in the Laboratory ceased after a time to afford adequate relief. Also, as years passed, the inelasticity of the time-table produced by the existence at the Laboratory of only a single course of lectures for Part I. began to be felt as an increasingly serious drawback. Now the work is divided between two courses, by Wood and Whetham, held on alternate days, and a new course of demonstrations, given by Spens for Searle, has been introduced on days which alternate with those of the course by Searle and Bedford. Thus not only are the numbers of students attending the various practical classes reduced to a less inconvenient size, but a greater freedom of choice of the days and hours of attendance both at lectures and practical work can be given, no inconsiderable benefit when it is remembered that every candidate is at this stage studying at least two other sciences at the same time as Physics.

Again, it will be observed that not only all the demonstrations but also practically all the lectures in Physics are now held within the Laboratory itself. This development, only rendered possible by the increased accommodation

furnished by the new wing opened by Lord Rayleigh in 1908, is full of interest. In the earlier years of the period under review that very substantial proportion of the teaching which has always been provided by various colleges through their official lecturers was in general given within the college precincts. Ordinarily the place was dictated by necessity and not by choice, though in the case of first M.B. students the fact that the college could thus easily supervise the work of its own undergraduates was considered very important. But such a system laboured under obvious disadvantages. It hindered that close contact between the lecture teaching and the practical classes which is so vital, and even the lectures could with difficulty be illustrated by experiment, as apparatus had either to be extorted from a naturally unwilling college, or else provided by the lecturer himself from his own frugal store.

At present, in the case of the first M.B. candidates, with the exception of a few revision courses, the whole of the lecturing is concentrated in the Laboratory, while those colleges which recognise the benefits of personal tuition and supervision to men at this stage of their career have provided assistant tutors who keep themselves in touch with the lecturer and give their students such assistance and direction as they find necessary during the progress of the course.

Practically all the rest of the lectures, whether given by University teachers or college lecturers, have now been brought within the Laboratory, its assistants and apparatus are at the disposal of the lecturers for the experimental illustration of their subject, and not only are the due co-ordination of the classes and co-operation of the teachers rendered infinitely more easy, but a sense of comradeship is fostered, upon the value of which it would be superfluous to insist.

We have now brought our sketch of the development of

the teaching of Physics in Cambridge down to the present day. When an organism exhibits not only continuous growth but also a ready adaptability to the varying demands upon it, we are justified in claiming for it a vigorous life. Those of us who have been teachers at the Laboratory have good reason to feel very proud of our students; those of us who have also been students have even better reason to feel very proud of our teachers, whose traditions we have endeavoured to the best of our ability to interpret to succeeding generations.

APPENDIX TO CHAPTER IX.

The following list gives the names of all those who have been appointed by the University to teach Physics in the Cavendish Laboratory, and the University terms during which they have held office. Limitations of space have made it necessary to restrict the list to those officially appointed by the University.

Professors.

James Clerk Maxwell .	Lent 1871—Michaelmas 1879.
John William Strutt, Baron Rayleigh .	Michaelmas 1879 — Michaelmas 1884.
John Joseph Thomson .	Michaelmas 1884—

Assistant Directors of the Laboratory.

R. T. Glazebrook . .	Easter 1891—Easter 1898.
W. N. Shaw	Easter 1899—Lent 1900.

University Lecturers in Physics.

W. N. Shaw	Easter 1887—Lent 1900.
L. R. Wilberforce . .	Easter 1900.
G. F. C. Searle . . .	Michaelmas 1900—
C. T. R. Wilson . . .	Lent 1901—

Demonstrators.

W. Garnett	Lent 1874—Lent 1880.
R. T. Glazebrook	Michaelmas 1880—Lent 1891.
W. N. Shaw	Michaelmas 1880—Easter 1887.
H. F. Newall	Michaelmas 1887—Easter 1890.
L. R. Wilberforce	Lent 1891—Easter 1900.
G. F. C. Searle	Easter 1891—
S. Skinner	Easter 1891—Lent 1904.
C. T. R. Wilson	Michaelmas 1900—
P. V. Bevan	Michaelmas 1904—Michaelmas 1908.
A. Wood	Lent 1909—

Assistant Demonstrators.

J. H. Randell	Lent 1884—Easter 1887.
J. C. McConnel	Lent 1884—Easter 1885.
R. Threlfall	Michaelmas 1885—Easter 1886.
H. F. Newall	Michaelmas 1886—Easter 1887.
H. L. Callendar	Michaelmas 1887—Easter 1888.
L. R. Wilberforce	Michaelmas 1887—Michaelmas 1890.
T. C. Fitzpatrick	Michaelmas 1888—Michaelmas 1906.
R. S. Cole	Michaelmas 1890—Lent 1892.
C. E. Ashford	Easter 1892.
W. C. D. Whetham	Michaelmas 1892—Lent 1894.
J. W. Capstick	Easter 1894—Lent 1896.
P. E. Bateman	Easter 1896—Easter 1899.
R. G. K. Lempfert	Michaelmas 1899—Easter 1900.
J. S. Townsend	Michaelmas 1900—Easter 1901.
P. V. Bevan	Michaelmas 1901—Easter 1904.
C. Chittock	Michaelmas 1904—Lent 1909.
T. G. Bedford	Lent 1907—
G. I. Taylor	Easter 1909—

A LIST OF MEMOIRS CONTAINING AN ACCOUNT OF WORK DONE IN THE CAVENDISH LABORATORY

[The dates are those on which the memoirs were published, and are, in a few cases, considerably later than those on which the work was finished. Only those publications are included which contain a considerable amount of original matter not published previously. In a small number of instances the later portion of the research was completed after the author had left the Laboratory.]

1874

J. C. MAXWELL.

Plateau on Soap Bubbles. *Nature, Vol. X.*

Grove's 'Correlation of Physical Forces,' *Nature, Vol. X.*

On the Application of Kirchhoff's Rules for Electric Circuits to the Solution of a Geometrical Problem. *Nature, Vol. X.*

Van der Waals on the Continuity of the Gaseous and Liquid States. *Nature, Vol. X.*

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On Double Refraction in a Viscous Fluid in Motion. *Proc. Roy. Soc., Vol. XXII.*

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A LIST OF THOSE WHO HAVE CARRIED OUT RESEARCHES AT THE CAVENDISH LABORATORY.

The information is compiled for the most part from answers returned to a circular letter addressed to all those who could be reached, containing questions on the following points among others:

(1) Previous university or college, if any;
(2) College at Cambridge;
(3) Present situation or occupation.

The replies to these questions are separated by semicolons.

The dates in brackets after each name are those between which the person concerned worked in the Laboratory.

ADAMS, EDWIN PLIMPTON (1902-3), Harvard University; Trinity College; Professor of Physics, Princeton University.

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ANDERSON, ALEXANDER (1885), Sidney Sussex College; Principal, Queen's College, Galway.

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BAKER, WILLIAM COOMBS (1900-2), Queen's University, Kingston, Ontario; Non-Collegiate; Assistant Professor of Physics, School of Mining, Queen's University, Kingston, Ontario.

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BRAND, A. (1891), Pembroke College; Barrister-at-Law, Lincoln's Inn.

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